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ABSTRACT

This document contains the proceedings of a workshop conference sponsored by the American Association for the Advancement of Science (AAAS) held at the Institute of Man and Science in Rensselaerville, New York, in May 1976. The proceedings include presented papers, discussions, and comments of the participants. The conference had two sets of workshop sessions and discussions of how science may be more effectively applied to social needs. Plenary papers described aspects of interprofessional collaboration in problem areas such as energy, transportation and applied social science research. Other papers considered ways in which scientific and other professional expertise was used in problem-oriented research and study. The four appendices at the end contain: (1) names and addresses of participants; (2) agenda; (3) evaluation; and (4) bibliography. (GA)

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Adapting Science to Social Needs

Conference Proceedings

Edited by
Richard A. Scribner
Rosemary A. Chalk

American Association for the Advancement of Science
1776 Massachusetts Avenue, N.W., Washington, D. C., 20036

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ADAPTING SCIENCE TO SOCIAL NEEDS:
KNOWLEDGE, INSTITUTIONS, PEOPLE INTO ACTION

Proceedings of a Workshop Conference
on Problem-Oriented Research

Edited by

Richard A. Scribner
and
Rosemary A. Chalk

Office of Special Programs

AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE
1776 Massachusetts Avenue, N.W.
Washington, D.C. 20036

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PREFACE

In May of 1976, the American Association for the Advancement of Science (AAAS) sponsored a three-day conference entitled "Adapting Science to Social Needs: Knowledge, Institutions, and People into Action." The Conference was held at the Institute of Man and Science in Rensselaerville, New York. This document is the proceedings of that conference.

The genesis of the conference may be found in discussions and activities over the period 1971-74 of the AAAS Committee on Science in the Promotion of Human Welfare (1961-1974). However, the impetus for the conference derives from a continuing widespread concern about the lack of ongoing examinations of the problems and prospects of public problem-oriented interdisciplinary research. The Office of Special Programs of the AAAS perceived a need to assess the dimensions of such examinations, as well as to evaluate potential contributory roles for the AAAS.

Discussions of how science may be more effectively applied to social needs have occurred within numerous groups. Frequently, the discussions have blurred the various conceptual, systemic, management, institutional, and "market" or user factors. For our conference, initial planning and development guidance were provided by an informal group consisting of the following individuals: Clark Abt, Abt Associates; Sherry Arnstein, National Center for Health Services Research; William Bevan, Duke University; Richard H. Bolt, Bolt, Beranek and Newman; Richard A. Carpenter, National Academy of Sciences; Kenneth W. Heathington, University of Tennessee; Don E. Kash, University of Oklahoma; Arie Lewin, Duke University; John McKinney, Duke University; Claire Nader; David Rose, Massachusetts Institute of Technology; Charles P. Wolf, City University of New York. Numerous other people also provided useful advice.

The keynote speech by C. West Churchman addressed the holistic nature of public problems and the reductionist approach of science. Plenary papers described aspects of interprofessional collaboration in problem areas (energy, transportation, and applied social science research). Other papers considered ways in which scientific and other professional expertise was used in problem-oriented research and study. Through the first set of workshop sessions, conference participants assessed such topics relating to the conceptual, individual, management, and organizational approaches to more effective utilization of scientific knowledge in problem-oriented efforts. The second set focused on recommending actions and changes.

While a mapping of the steps involved in moving from the specification of the problematic to implementation flows from information to action, the final steps which include policy implementation are, strictly speaking, outside the domain of this conference. As one participant in the early planning discussions stressed, the principal purpose should be "how we (the professional communities in various institutional environments) can get our act together" to contribute more effectively.

A preconference document, distributed before the conference convened, contained drafts of nearly all of the presentations as well as brief statements from the workshop chairmen and a set of initial questions for each of the workshops. The existence of the document in the hands of each participant not only provided a common base and enabled each speaker to address areas of principal or provocative interest, but also gave the participants a ready reference resource during the conference. Participants selected the workshops they wished to attend. More than 70 persons from the public and private sectors -- universities, research organizations, corporations, and government agencies -- including specialists in the natural and social sciences, policy sciences, management science, public administration, and the humanities participated. Participants included people experienced in performing interdisciplinary problem-oriented work; managing such projects; developing programs sponsoring such work; or using this kind of research in problem-solving efforts.

The conference was partially supported by the Office of Extramural Research, National Institute of Mental Health and the Research Management Improvement (RMI) Program of the National Science Foundation. Some of the RMI Program supported recent studies examining conditions for effective university-based interdisciplinary efforts were included in plenary and workshop sessions. The proceedings document and other dissemination efforts are supported in part by the Division of Policy Research and Analysis of the National Science Foundation.

We acknowledge the generous assistance and advice of the members of our informal planning group and many others. We thank all the conference participants for their many contributions and for enduring the three conference days with such good cheer. We thank Robert Cutler and Ernie Powers of NSF, and especially Betty Pickett of NIMH. We acknowledge the many contributions of Mary Dolan, Catherine Tafoya, Jeannette Rehbock, Albert Wright, and Ralph Manual.

And finally we thank Gordon Enk, of the Institute of Man and Science, for his advice and help, and to his staff and colleagues for their substantial contributions to the success of our conference.

We hope that all who participated and all who read this document not only will be informed, but also will be stimulated in some way to contribute further to this important area.

Richard A. Scribner
Manager, Special Programs

Rosemary A. Chalk
Conference Coordinator

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"The view that interdisciplinary research teams are needed to adapt science to social problems reflects a growing belief that in a technological society the social and physical systems are inseparable."

Don Kash

INTRODUCTION

Richard A. Scribner
American Association for the Advancement of Science

The title of this conference, Adapting Science to Social Needs, was consciously and deliberately chosen. It is not a precisely accurate connection of words, for clearly science cannot itself be adapted to anything and still remain science. But the title hopefully conveys concisely the thought that knowledge institutions and the manner in which some scientists and other "knowledge producers" do their work can and should be changed in ways to contribute more effectively to solving or ameliorating pressing social problems. Both of these assumptions are admittedly arguable -- and we will no doubt argue about them at times during the next three days -- but they are where I begin.

Assuming we can and should make these changes, the questions before us include: What are the present barriers to more effective, holistic contributions? How can we better understand what needs to be done? and What are some useful steps to move us further toward more effective contributions?

BACKGROUND

During the past ten years, we have seen widespread efforts to better utilize science and technology for the improvement of human welfare. These efforts would use science for better development, management and protection of our resources, for amelioration of pressing social problems such as drug abuse and crime prevention, for alleviation of the problems of urban development, and so on. As efforts continue, one observation surfaces and resurfaces: our scientific institutions generally lack the capacity to effectively turn knowledge to action.

As Harvey Brooks has noted:

> Today all the advanced industrial nations are suffering from disillusionment with the failure of science in attacking what are perceived as the most urgent problems of the future. It is increasingly appreciated that the blame lies not so much with science itself as with the lack of mechanisms and institutions to couple science to its operational applications in the social sphere. 1/

Pursuit of these efforts has generally proceeded on the assumption that science and technology have the capability to contribute substantially toward the solution of some pressing social problems, provided that properly directed research and adequately supportive institution-building are accomplished. Some have questioned this basic assumption and the purely "organizational" approach arguing that there are inherent limits to the application of disciplinary or even multidisciplinary, reductionist thinking when solving complex public problems. A more holistic, truly interdisciplinary or interprofessional approach to problem-solving is required, according to this school of thought. However, present modest attempts to do interdisciplinary work of this sort are neither easy nor necessarily successful. Several reports have noted the difficulties seemingly inherent in defining, sponsoring, performing, and utilizing interdisciplinary public problem-oriented work.

Within this area, lack of understanding about the management, communication, and other factors affecting interdisciplinary research is striking. Other factors include the lack of integration and utilization of applicable science knowledge, and the still widespread lack of communication between natural and social scientists. Since it seems more and more evident that interprofessional collaboration is needed to effectively address the use of scientific knowledge for solving social problems, it is clearly necessary to examine the accomplishments and shortcomings of attempts to date. Greater and more effective adaptation of scientific knowledge and methods, and better utilization of the results of interdisciplinary, problem-oriented work will require more understanding of the circumstances of the applicability for these approaches and strategies for overcoming systemic obstacles.

A report prepared by the Committee on Public Engineering Policy (COPEP) at NAE in 1973 highlighted the need for a study of this sort. The report, titled Priorities for Research Applied to National Needs, stated in the concluding section:

> Concern has been expressed...that the nation's capability to undertake and utilize the kind of research recommended herein may be limited, particularly where there is dependence on the use of social science personnel for work in problem-oriented areas of research. These and many related questions were discussed during this study, but...such problems are so extensive and important as to merit a study of their own. 2/

A second report recently prepared by the Center for Research on Utilization of Scientific Knowledge at the University of Michigan indicates in its conclusions that new kinds of institutional arrangements which encourage interdisciplinary interactions are needed if effective utilization of social science research is to be achieved. The report states:

> Insofar as the producer and user communities are comprised of individuals with differing abilities and inclinations to deal with the scientific and extra-scientific aspects of policy issues, effective utilization probably will proceed best if it is pursued by a set of individuals representing different combinations of roles and skills who are located in an institutional arrangement which allows them to take into account the practical factors affecting both the production and the use of knowledge. 3/

Today, more than three years after the preparation of the COPEP/RANN report cited above, there is still widespread and acknowledged concern regarding the lack of an ongoing examination of the problems and prospects of interdisciplinary research.

ON ADAPTING SCIENCE TO SOCIAL NEED

The attempt to apply or adapt science and engineering to social needs takes many forms. It is, for example, concerned with using the knowledge and methods of science to anticipate probably beneficial and detrimental effects of certain technological advances or courses of action -- technology assessment, in other words -- it is concerned with the development of means whereby the scientific and engineering community can better understand the needs of public problem-solvers and policy-makers; it is concerned with the framing of "disciplinary" questions in order that they have maximum relevance to, and therefore presumably also greater use in understanding and solving actual social problems and, for a final example, it is concerned with how the full breadth of scientific and engineering (and other professional) expertise can be brought to bear in examining possible solutions to our public problems.

Generally speaking, our approaches to solving pressing national problems are fragmented and sometimes counterproductive. Effective holistic, rather than more elaborate reductionist approaches are required. For example, in writing on the energy research area, David Rose has commented:

> ...advances in understanding come at the synaptic points where various disciplinary specialties interact; optimizing along any single disciplinary

> line is no solution to the problem. Major problems cannot be properly defined in terms of single specific tasks; nor is it enough to think of the sums of such separate tasks. If one attempts to divide a problem into its separate parts -- by whatever rules -- some connective tissue will be left over, which cannot be ignored or separately assigned. 4/

Frequently attempts to bring scientific information and analysis to bear on social problems are lumped under such rubrics as "research applied to public problems" or even more mysteriously "problem-oriented research." Basic researchers in the natural sciences, corporate research management specialists, and keepers of semantics wince at such terminology. However, when efforts of analysis and knowledge generation take place for purposes more oriented toward fulfilling or facilitating the mission or goals of social institutions or agencies, it is generally termed applied or policy-oriented research.

This work takes place in many institutional settings: in universities, in corporate settings, in nonprofit- and profit-making research institutes, in national laboratories, and in government agencies and under the auspices of the National Academy of Sciences and professional organizations. The relationships and interconnections that concern us are those between, for example, government policy and university structure; mission agencies and research units, mission agency heads and leaders of problem-oriented teams; between the Office of Technology Assessment and its contractors, and between universities (or "knowledge centers") and local institutions which are attempting to solve, say, local urban problems. The processes which concern us involve, for example, the approaches of the scientific groups to the application of (relatively compartmentalized) knowledge to particular social problems, the involvement of "users" in shaping the approach, the supportive or nonsupportive structure in which the work is to take place, and the means through which the usefulness of such efforts may be enhanced.

INTERDISCIPLINARY POLICY RESEARCH

One major approach toward adapting scientific knowledge to public problem-solving is the so-called interdisciplinary policy research. This research occurs in different environments -- the universities, government, industry, and independent research centers -- but most often in academic settings. (Although, as some observers have noted, if current shifts from research grant to contract-type work continues, before too long few universities will be able to do large research projects affecting the big questions in social research.)

Interdisciplinary research management in the university environment has been the subject of considerable attention. 5/ Similarly the problems of conducting such research and the necessary conditions for the nurturing of it have also received much attention.

Nilles has noted:

> Interdisciplinary research is simultaneously an old and a new phenomenon on university campuses. The growth of knowledge has always been a process of building upon past experience of some aspect of life or thought, borrowing investigative tools from other disciplines and thereby developing both new areas of knowledge and new means of exploration. This borrowing is often an interdisciplinary process and is quite frequent in professional schools such as law, engineering, medicine, and business administration. Yet, interdisciplinary research is also new in a very real sense. Man's increasing use of technology in particular has brought about a growing recognition that the world consists of a myriad of complex and interrelated components. Changes in our use of irreplacable resources, alterations in the way we communicate, provide health care or move ourselves from point to point, all have far reaching and often, in the past, unforeseen societal effects. Thus the interdisciplinary research which is becoming increasingly important in meeting these challenges must include cohesive consideration of a wide variety of societal and technological factors. These can only be integrated by combining the talents of a broad array of researchers. 5/

Nilles states that the term interdisciplinary research implies the joint, coordinated, and continuously integrated research done by experts with disciplinary backgrounds, working together and producing joint reports, papers, recommendations, and/or plans, which are so tightly and thoroughly interwoven that the specific contributions of each researcher tend to be obscured by the joint product. It involves the interaction of clearly nonadjacent disciplines such as the physical sciences with the social sciences and/or law, economics, etc. It differs from multidisciplinary research in that the latter can be performed by experts with different disciplinary backgrounds, but who work separately, not necessarily in the same environment or with any mutual consultation, exploring different aspects of a central problem.

The distinction is not semantic or trivial. Interdisciplinary research, with its implications of continuous integration and refinement about common central issues, is presumably more useful. The experience both of universities, corporations, and funding agencies to date has been that interdisciplinary research occurs much less frequently than could be desired. 4/

The focus on interdisciplinary research often leaves the impression on people not directly involved that the approach to policy research has become the end goal. Clearly, such an approach is but one way (perhaps one of the best ways) for disciplinary knowledge to be integrated in problem-oriented work. From that perspective, perhaps the principal criterion of success in interdisciplinary policy research should be its ultimate utility.

Some people have raised the question of whether universities should be involved in problem-oriented research at all, while others indicate tremendous potential for using this research as the basis for new training programs and curricula aimed toward real world problems. Derek Bok, President of Harvard University, raised this question of the proper role for the universities during the 1976 AAAS meeting in Boston:

> If research moves toward large-scale team efforts to attack immediate national problems, we will finally have to grapple seriously with the question of whether universities offer the best environment for work of this kind. In the past, universities have provided an ideal setting for research because they offer detachment, independence, security, and an opportunity to conduct research and train a new generation of investigators through a process whereby training and research each reinforce the other. But it is far from clear that these virtues will apply in equal measure to the conduct of large-scale problem oriented research. In work of this kind, independence and detachment may actually hamper the development of closely coordinated interdisciplinary work. And it is questionable whether highly structured, applied research offers an appropriate setting for training graduate students. All in all, the nature of this work is sufficiently different from the traditional work of universities that thoughtful educators will need to consider to what extent their institutions should try to participate. 6/

And if the universities do choose to participate:

> ...they will clearly need to study a number of subtle problems of institutional architecture -- how to induce experts from different disciplines to work together, how to make certain that interdisciplinary

work is constantly renewed by interaction with its parent disciplines, how to develop the new modes of authority and governance required to coordinate a team effort, how to provide the incentives and criteria for advancement that will attract able young investigators to cast their lot with these new undertakings. 6/

While some of our concerns and approaches are most often thought of in a university context, such an environment for adapting science to social needs is not the only concern of this conference. Nor is our focus exclusively on interdisciplinary problem-oriented research. Rather these are principal foci in a larger, interconnected array of institutional environments. The broader concern is how to more effectively couple the results and methods of science and engineering to the solution of public problems and the policy process.

CONFERENCE PURPOSES

The workshop-conference has the following purposes:

1. To examine some of the present modes through which scientific, engineering and other professional expertise is used to perform problem-oriented research;
2. To better define the barriers or weaknesses within this enterprise which prevent or inhibit more effective or innovative approaches;
3. To assess the areas of continuing uncertainty and confusion regarding more effective modes of better adapting science and engineering to social needs;
4. To suggest institutional, system, and behavioral changes which could help to make these efforts more effective and useful; and
5. To recommend courses of action for AAAS and other organizations.

The conference is built upon a core of information presented in the following sequence: describing the requirements for scientific knowledge and research in specific problem areas (such as energy, social problems, and transportation); assessing the development of some management and organizational processes for performing interprofessional, problem-oriented research; and examining some factors affecting the utilization of research work in social problem-solving.

ANTICIPATED OUTCOMES

The best and most comforting view of this or any other conference is as one event in a process. It is expected that some things set in motion by the conference will be continued by the AAAS -- some presentations may become articles in Science and other publications. However, it is always difficult to project precisely what the outcome of a conference will be or what activities will be occupying further attention. Nevertheless, some probable results can be suggested.

The conference discussions and the subsequent refinement of the proceedings should highlight some unresolved questions, approaches to answering them, and suggested agendas for action by various organizations. For example, the conference may develop suggestions and guidance for mission agencies and supporters of interdisciplinary research for further examining the effects their posture and contractual arrangements may have on the motivation and reward structures within research performing institutions and on the usefulness of the results. The conference may suggest that a national university professional association give additional serious consideration to ways in which disciplinary structures and interdisciplinary efforts can more easily coexist and be mutually supportive.

The workshops may produce recommendations for facilitating the linkage of broad-based interprofessional groups or knowledge centers with public problem-solving and policy formulating bodies. They may come forth with plans for professional organizations to carry out programs designed to further legitimize policy-relevant research; suggestions for the AAAS to become involved in collaborative endeavors with other appropriate professional organizations in examining conceptual, institutional and organizational questions; plans for "programs of education" to other communities who should be aware of some of the input to and results of this conference.

In developing the background for this conference, some of the persons we contacted expressed surprise that the AAAS was involved in a project related to managerial and organizational processes. However, the Association is in the midst of rethinking both its present science and public policy program and reframing its general, longer-range goals. The conference proceedings will be disseminated widely to persons in the private and public sectors who have been identified as interested in the overall objectives of the conference. The results suggesting a more active role within AAAS sill be integrated with the ongoing planning process within the Association. What is clear already, is that the AAAS will strengthen its role as a center for interdisciplinary communication, move toward fostering more effective interdisciplinary research and interprofessional endeavors, and seek to take greater advantage of the leverage extant in its unique institutional role. In that context, one area of major concern for the Association *is* how to better adapt the knowledge and methods of the sciences and other professions to the needs of public problem solving.

Finally, since the conference is a "point event" and, as already noted, we *are* involved in the encouragement and strengthening of a process, the greatest output may be that which results from what you, the participants, and others can do in carrying on from this point.

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1/ Brooks, Harvey, "Knowledge and Action: The Dilemma of Science Policy in the '70's," *Daedalus* (The Search for Knowledge, Spring 1973), p. 125.

2/ U.S. National Academy of Engineering, Committee on Public Engineering Policy, *Priorities for Research Applied to National Needs*, 1973.

3/ Caplan, Nathan, *et al.*, *The Use of Social Science Knowledge in Policy Decisions at the National Level*, A Report to Respondents, Institute of Social Research, The University of Michigan, Ann Arbor, Michigan (1975), p. 25.

4/ Rose, David J., "New Laboratories for Old," *Daedalus*, Summer 1974 (Science and Its Public: The Changing Relationship), p. 143.

5/ See, for example, Nilles, Jack M., "Interdisciplinary Research Management in the University Environment", *Journal of the Society of Research Administrators* (Spring 1975), p. 143.

6/ Bok, Derek C., "Universities and National Research Policy," remarks delivered at the Annual Meeting of the American Association for the Advancement of Science, February 18, 1976.

" ... for science to contribute in the way that we hope it would, scientists and the system that maintains them must become holistic in a far deeper sense than merely by being interdisciplinary."

Donald Michael

I.

KEYNOTE ADDRESS

"Does the knowledge system work well? The knowledge generators are generally satisfied. The managers in the federal government ... are seriously worried. The state and local government people (feel that the) system worked very badly .. as for the public, ... they were not heard from ....

... something is wrong if the generators of knowledge in our disciplines are very specific, and yet the need for information is holistic."

C. West Churchman

TOWARDS A HOLISTIC APPROACH

C. West Churchman

School of Business Administration
University of California, Berkeley

I would like to begin by relating this conference to another one. Last week I attended the National Science Foundation's second forum of their bicentennial year celebration which has the peculiar title "Project Knowledge: 2000." Its intent was to look at the knowledge system and its relationship to other aspects of society. It was based on a flow model of the way the knowledge system works.

This system begins with an identified need on the part of society; it then flows to people who have the capability of funding knowledge investigations which will respond to that identified need (they are people in various aspects of government: state, local,and federal). It then flows to the generators of knowledge, the third group, who may indeed be involved in the first step in identifying the need. Then, eventually, that knowledge is supposed to flow into the users, the people who benefit from that knowledge, who then generate a demand for new needs. Those users of knowledge actually represent a backflow, because they are the people who fund the knowledge generators and also help the managers decide which of these generators shall be funded.

If step one, the identified need, is eliminated, then one gets something called basic knowledge, or basic research, where the identified need occurs among the group of knowledge generators themselves and it is not related directly to any identified need on the part of the public.

In Project Knowledge: 2000, the conference was organized in essentially six teams, who were identified by the kinds of individuals they were in the flow model I described. There was, for example, representing the identified need, a group of state and local government people, ranging from people in state legislatures as well as boards of supervisors or city governments. There was also a public interest group, which was a team respresenting people interested in labor problems, a girl representing the National Student Association, a woman who was President of the Women's League of Voters, and so on. The second group of people, who represented the managers and funders of knowledge, were staffs in government departments, mainly federal, such as NIH, NSF, ERDA, Labor and Interior, and some congressional staff. The third group were the generators of knowledge, in which there were three categories: universities and colleges that do research, the think-tank types such as RAND and SRI, and the private corporations and their research interests.

The fourth group, the users of knowledge and ultimate recipients of the benefit of the knowledge-generating system, the public (although that meaning was certainly not clear at this conference) was not represented by any group directly. In other words, the conference did not go out into the streets of Leesburg, Virginia,and ask citizens to come and form another group.

I thought that the basic question of the second forum of Project Knowledge: 2000, was:

"Does the knowledge system work well?"

In effect, since the public is paying for the knowledge system as we have it today, can you say that the system pays back the funders well and satisfies the basic needs that are generated? Secondly, how well will the knowledge system work in the year 2000?

The responses were interesting.

A. The knowledge generators are generally satisfied that the knowledge system does indeed work well. They recognize that certain cautions have to be made, and that one must expect changes in the knowledge system in the coming decades. In fact, David Apter, a well-known political scientist, pointed out that in his experience all universities are constantly in change. The fact that they are constantly in change in a minor way didn't disturb him at all.

B. The managers, the people in the Federal Government (although they could also be in state and local government), who have to decide on the allocation of funds and, in particular, recommend to their bosses policies as to the allocation of funds for supporting research, are seriously worried. They do not think the system works well. They feel that there is real difficulty in understanding what the knowledge-generating community is trying to do. They are trying to evaluate and make recommendations to their bosses through memoranda but their bosses are very busy people who have many other things concerning them.

C. The state and local government people felt that the knowledge system worked very badly from their point of view. It does not respond to their needs for knowledge in deciding the various issues that they face in counties, in cities, and in states. They were deeply dissatisfied with the present knowledge system.

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D. As for the public, the eventual users, they were not heard from in the conference.

ESOTERIC AND EXOTERIC KNOWLEDGE

Now for a speculative interlude. As a philosopher I am interested in the nature of knowledge and I would like to discuss the various forms that knowledge has taken in the history of the human race. The scientists can be divided into two types: those that are interested in esoteric knowledge, essentially knowledge directed to people of your own kind who understand the particular form of the knowledge in certain terms (symbolic logic is a good example); and those that are interested in exoteric knowledge. Exoteric knowledge means knowledge that is advantageous to the public.

There are two forms of exoteric knowledge. One flows directly from esoteric and the other does not. For instance, some friends of mine are trying to build a fence around a garden which is about fifteen yards long and five yards wide, and they have the question of how much fencing material is required to fence in the garden. The knowledge of how much fencing material is required flows from the esoteric science of metric geometry and it can easily be calculated. On the other hand, in the state of California at the present time, we are considering changing the building code so as to permit people to build their houses out of wood that they have obtained from old barns. The building code as it exists now does not permit construction of that kind. The county of Mendicino would like to consider changing the building code (I'm now back to my state and local government people in this example).

Now consider the difficulties of that problem. In the first place there are contractors who have been building according to the building code and would like to continue to do so since that's profitable. There are old age people who have been made to pay for houses a higher amount than they expected, because their houses were built "up to the code." There are the younger people who have taken the building materials from the various barns (the lumber from which is probably better than any current crop). These are some of the people involved in this problem. It's not like a problem with a perimeter of a certain area. It comes out to be a problem of how those various groups are to be served in something as minor as changing the building code of the state. Exoteric knowledge begins not to look like esoteric knowledge in that context at all.

Responding to the state and local government team of Project Knowledge: 2000, I would like to say that they recognize that today's decision-makers on the boards of supervisors and in city councils and State governments have to be holistic. They are forced to recognize the holism of the problem. That is to say, they have to sweep into their considerations all those different human values and complexities that are representative of the problem, even one so "small" as just changing one aspect of a building code. And that's just one of a thousand things that they have to consider every week. They struggle to get all that is relevant in order somehow to arrive at a decision, because that's their job.

Of course, we all know that the politicians in their speeches remain simplistic. Gerry Ford says that we must be energy independent in 1984. He doesn't mention the fact that to accomplish this involves fantastic complexities in the rearrangement of our social conditions, in education, in the sacrifice we'll have to make in various health programs, in various kinds of recreational programs, and so on. The politician can come out with a very simplistic, reductionist statement even though he knows very well that the ultimate decision will have to be based on holistic considerations.

Thus something is wrong if the generators of knowledge in our disciplines are very specific, and yet the need for information is holistic. Hence, there is a mismatch between esoteric and exoteric (needed) knowledge. Some realization of this state of affairs has occurred in economics. The "old men," very much like the old men of the tribe, including Leontiev, Aaron Gordon, and others point their fingers at the younger people, saying, "You are doing nothing but building irrelevant models. They spawn inadequate information. The big problem is how to get adequate information." That, to be sure. We ought to make our economic data bases much better. But, even if we do, we haven't really addressed the problem that faces the decision-maker in state, local and federal government.

There are really differences in knowledge-generating systems. The university represents to me the most esoteric kind of system. We divide ourselves into disciplines, and we try to generate knowledge that's given an "excellence-mediocrity" score by the peer groups within the discipline. On the other hand, there are certainly other kinds of knowledge-generating systems that work through consultation, like SRI, A. D. Little, and so on, and the think-tanks, which are not so oriented to the disciplines. There are also the private corporations who are interested in developing products and services based on science in which they recognize the need to be interdisciplinary.

DISCIPLINE-ORIENTED RESEARCH MAY WORK POORLY

My remaining comments are concerned with nonholistic discipline-oriented research, which, in terms of percentages, is not a big part of the knowledge-generating system, although I have a feeling that even consulting companies and think-tanks are vastly influenced by discipline-oriented research. I have five reasons, which are all the same, why discipline-oriented research may work poorly in the knowledge system and hence its future funding may not be appropriate. We're facing a real challenge to the discipline-oriented research system. The challenge will become more and more serious, primarily because the money for such research is coming from the public. We no longer have a Duke of Somebody or a very rich Ford Foundation; we will have to rely more and more on public institutions to fund our scientific research and as that happens I have a strong feeling people will begin to question why they are funding the kind of research we conduct.

On the very last day of Project Knowledge: 2000, there was a confrontation between one of the public interest people, from the Women's League of Voters, and a famous physicist. He said to her, "I don't want you getting down into the guts of my research. It's all right if you are interested in the general policies of funding a certain area of physical research, but I don't want you involved in why my particular kind of research is valuable, because you can't possibly understand it."

And she said, "If I can't understand it, why should I fund it?"

He replied, "You couldn't possibly understand it; it's much too complicated. I want you out of the guts of the system."

She was saying, "I want to be in the guts of the system because it's eating up a lot of my funds."

I think the issue is a much more serious one than should the scientific community spend some of its time worrying about social needs. I think more and more the public is going to ask, why should it fund any of the basic research effort unless it can show why it is serving social needs. I mean my remarks to be disagreeable, by which I mean I hope that disagreement will happen in our work sessions. If it doesn't and we spend our time trying to agree with each other, then the days will be wasted.

There are five reasons, all the same in character, why disciplinary-oriented research may work poorly in the knowledge system; and,hence, its future funding may not be regarded as appropriate by those who supply the funds.

First, the disciplines themselves. We've inherited the disciplines from the 19th century. Consider research in the 18th century and take Immanuel Kant as an example. I can just imagine the head of a disciplinary department calling in Kant and saying, "Now look Kant, you really have been spreading yourself much too wide. You've been working on the origin of the universe, on the fundamental nature of knowledge, and on certain physical problems and then on deep psychological problems as in your latest book, The Foundation of the Metaphysics of Morals. I really think you ought, you know, in order that your career develops correctly, narrow in on one of these and let the rest go. You've been much too broad in your approach." That could not be said to Kant or Leibnitz in the 17th or 18th centuries. But the 19th century gave us the disciplines -- the ways in which we began to narrow in and segment various areas of knowledge.

So, we became interested in the physical nature of the universe purely from the point of view of the physical nature; we are not interested in whether the physical nature of the universe is related somehow to the psychological nature of ourselves. I'll just point to Kant's statement at the end of The Critique of Practical Reason: "Two things fill my heart with never ending awe . . . the moral law within and the starry Heavens above." Kant did not see that these were separate. They were for him the same kind of problem. The immensity of the universe comes from the same kind of psychological being as the nature of morality. But you see what the 19th century did. It snipped. Morality was one thing and the physical universe was another. Now I think we've got to go back, be reactionary and return to the 18th-century notion that those two things, the nature of the physcial universe and the nature of the psychic universe, are one and the same problem.

There are good reasons why the disciplines worked well. They served good political functions. They performed well in safeguarding the scientific community from all kinds of political attacks because one could design an esoteric community, and say that scientists were not involved in the problems that are associated with society and ethics. Thus "science" preserved itself well. But I hope that the era is over with.

We're coming to an age where we begin to question the real value of the disciplines and their associated reductionism, including the paradigm fads that drive a discipline so strongly. Economics in the past few decades is a good example of the change in paradigm. I'm using paradigm in the sense that Kuhn does in The Nature of Scientific Revolutions, but more specifically in terms of the history of philosophy. Disciplines pass from rationalism to empiricism, and back again.

One of the consequences of disciplinarity is what I call violence to the Ph.D's. We say to them, "You must carve out problems that are manageable according to the paradigm of the discipline you're in." What we should be saying to a Ph.D is "if you're inspired by a broad problem then try it! Don't narrow it! Broaden it! And, at the end of a period of time we'll look at what you've done and decide whether that warrants a Ph.D or not." Spirited Ph.D's want to study, say, how you make a city work. And then some son of a bitch comes along and says, "Now wait a minute, Tom, let's narrow that down. Maybe you want just to consider transportation. And maybe you don't want to consider the whole transportation system; maybe you just want to consider buses. Maybe you don't want to consider the whole of busing, but just busing in a certain district." And by that time the Ph.D is finished. His interest in the whole problem or the broader problem is gone.

It all amounts to an unwarranted emphasis on something called "excellence" in research, which is esoteric in character. I know that because I come from the University of California at Berkeley, where the basic theme of the academic community is excellence. Somebody did a survey and found out that our graduate program was the most excellent in the country, and that did us a great disservice in many ways. The contrast is between esoteric research and exoteric; esoteric research uses judgment of peers as its criticism, whereas exoteric uses relevance to social problems. Exoteric research may tend to be vague, non-excellent, difficult to evaluate, but perhaps helps somebody solve a problem.

Second: basic versus applied. We get basic research also from the 19th century, another one of our gifts, or, possibly, nongifts. Basic research meant, traditionally, research that looked at the foundation of knowledge. But we've come to learn, I think, that in some sense every discipline can claim to be basic in that sense. I'm trained as a logician. We have no difficulty defending ourselves as basic, because if you ain't logical, then you ain't nothing: if you're inconsistent, then anything follows. So we're basic. Mathematics is basic. Psychology is basic, isn't it? Because everything follows from the human psyche. If you don't understand the human psyche, you don't understand what science is about.

I remember once at the meeting of the faculty committee of the Health Science Program at Berkeley two biochemists leaned back and said, "after all, health is basically biochemistry." When I reported that to some of my friends at Donner Lab, they said, "Those chemists! They don't understand that structure is the important thing: it's basically biophysics." And so it goes. There is no problem in finding out that your own discipline is basic in every respect.

Another meaning of the word basic is "no specific application in mind." You work on the problem *per se*. I spent this weekend with a friend working on the theory of numbers (very fascinating) to prove that the fundamental theorem of arithmetic works in very general ways. Showing this doesn't really "help," and if I told you the generalization of the theory of numbers, you might wonder whether that helps you at all in improving the quality of life.

Now one defense of basic research is that eventually it does help people. However, anybody who is involved in management knows that if such a defense is offered for a project, then he should try to do some long-range planning, and see whether a supposed nonpurposive research effort really has future potential relative to human goals.

Finally, one could defend basic research by saying that some of us are geniuses and,therefore, you other people ought to support us no matter what the hell we're doing. Support us because we're bright and geniuses. That's a little more honest, in a way, than any other argument for basic research I've seen.

But I think we ought to break down the distinction between applied and basic. We ought to look at basic versus applied and consider them in terms of what both should mean -- in terms of exoteric knowledge.

Third, we have a word in esoteric science called "objective" (unbiased), which apparently means something to us. Indeed, I know that objective meant something very important to me in the past, because I've worked very hard trying to understand measurement. In World War II, I worked as a physical chemist and statistician. I tried very hard to get objective measurements of the properties of materials for the war effort. What this meant to me was that different laboratories should report in a similar way, independent of the individuals or the social structure of the laboratories. I might say that physical science was really very deficient in terms of that meaning of "objective." Many of the laboratories reported radically different measurements, and statistically showed an observer bias. Thus, one meaning of "objective" says, in effect, that different people, will all come to an agreement within limits, given a conscientious effort on their part as to how the measurement numbers will be generated.

Now I call your attention to the opposite of "objective," and that is *not* subjective. I know it's natural to think of objective, and then to think of subjective: "objective" means

I tell you how much this weighs, or what the distance is between two points. "Subjective" means I have an impression that this auditorium is beautiful. But that's not the contrast I want to make this evening. Rather, I want to make the contrast between "objective" and "judgmental." Judgment means that you make a decision in the context of a conflict of ideas, and there is no rational basis for resolving the conflict. There are no clear algorithms, or decision-making rules, or whatever, for resolving the differences. The word judgment, as well as decision-making, comes etymologically from the context of a decision made in the context of conflict. In fact, according to my Oxford dictionary, the word decision did not come to mean a lonely decision-maker kind of thing until about 1850 when Disraeli used it in that fashion. Before that it always meant a decision arrived at in the context of conflict -- of ideas, of interests, of values.

I go back to my state and local people. They are involved not in objective knowledge, but in judgment: they must use "judgment knowledge," knowledge that's created out of the various conflicts that the public represents. For example, my building code illustration is a decision based on judgment. A judgment is knowledge, but it's not the kind of knowledge that many of us scientists are familar with. It's not at all the kind of knowledge that we give in our disciplinary textbooks, where at the end of the chapter the exercises give the student enough information to arrive at an objective decision. Such a student finally goes out in the world and tries to use the information contained in the chapter, but finds that he's in a judgment situation and has no clear idea of how to apply what he has learned in these chapters. For example, in operations research, at the end of the chapter on inventories; we say, this is the information you need to know, the demand, the costs of holding and shortage, etc. The students go out and find that the data don't exist. They discover that the "data" have to be generated by judgment, based on a conflict of opinions. Thus "exoteric knowledge" comes mainly from "judgment knowledge."

Fourth: fragmentation of problems. One of my colleagues at Berkeley wants to "carve off" a piece of any problem he faces. I think he has a kind of a picture of the world of problems as a rather large roast beef in which one is asked to carve off suitable pieces one by one. Fragmentation means trying to break social reality into episodes in which we try to solve each episode in terms of its own characteristics.

An academic's life tends to be a series of papers and books that he has produced. Whereas, social reality is an interlinked continuum. To be sure, it's useful to break down the continuum into problems, but only for the sake of holding it for a moment -- much as you would if you were editing a film for the purpose of making it into a total film. You have segments of it, but you know these segments are not the reality. They're only ways of holding a piece of it for the moment to describe it, in order to get back to the continuous learning process.

Fifth: knowledge values versus ethical and moral values. The disciplines tend to emphasize one kind of ethical value which expresses itself in terms of truth, accuracy, reliability, and nontriviality. Those are the values that the knowledge community recognizes; it expects you to report truthfully, with all the accuracy and reliability at your command. And, of course, I expect you as a researcher, in order to attain excellence, to reach a level of non-triviality. But these values are only a part of the fabric of ethical values.

As I said earlier, throughout the history of a humanity that wrote down its ideas on human values, there is an incredible agreement as to what we humans are about. One label for our collective ideal today is "quality of life." Maybe a better word is "contentment," which comes from the Latin verb "contineo," meaning "to hold with." It means essentially, for each of us, would you rather have lived your life differently at the end?

The idea of contentment is illustrated by a horrible psychological study, in which the researchers said, in effect, "all right, you middle class Americans, you've come to the end of your life and God or St. Peter, whoever is in charge, says, you've got two choices: one is oblivion and one is to relive your life exactly as you did live it, without the knowledge that you've lived it. Which do you choose? Oblivion or going through the whole damn thing again?" And the great majority said "oblivion." If you believe the results, they say we middle class Americans are not content, in the sense that we want to hold onto our lives.

The ethical ideal is illustrated in the life of the hero in mythology who was a contented man despite the fact that he faced enormous odds against him. He went out and fought all kinds of monsters and beasts, and yet was content. That was the life he wanted to live. Contentment does not mean "peaceful around the fire"; it means that you want to "hold with" what is happening.

Another version of the ethical ideal in the history of thought is equity, so beautifully presented by Kant in The Foundations of Metaphysics and Morals, one of the greatest books, in my opinion, on human values, that has ever been printed, in which Kant describes the ideal of human equity:

"never treat another as means only, but as an end withal." Kant envisages a kingdom of ends in which everyone is a king and everyone is treating others as kings. We're living through Kant's ideal at the end of the 20th century in very marvelous ways. The ideal of equity has arisen again. It's one of the most beautiful concepts of ethical value that the human race has ever invented in terms of the women's movement, racism, so-called "developing nations," and so on. The ethical ideal is hard for the disciplinary community to grasp. They ask, "what's the evidence for this glorious grand idea of equity and contentment"? They have trouble with it, defining it, measuring it, understanding it, because much of it cannot be expressed in disciplinary language. "The greatest good for the greatest number" is not a very good way of expressing the concept of equity. Equity means treating every individual as a unique individual. But we don't have good disciplinary ways of describing uniqueness, which is a very ephemeral but very fundamental human feeling.

THE NEXT STEPS

You'll be glad to know I'm coming to a summary. A knowledge system, strongly oriented towards disciplinary, basic, objective, problem-oriented, excellent research, works poorly and will work even more poorly in the next decades in the future with respect to the betterment of the human condition.

Therefore, what we need to do is to explore (1) the conceptual meaning of holism, which I've only touched on this evening, and (2) the very important part of the inside of the knowledge-generating system, the reward system. Nowadays it works against those who are antidisciplinary in their orientation, and so we need to explore changes in the reward system so as to reward the heroes of holism who take on tasks much too big for them (they do not try to carve off the pieces), and enemies much more powerful than they are.

I'm reminded of an episode in my life when I served on the Council of the National Institute of Allergy and Infectious Diseases, in which Sabin told me that in the three years he'd served on that Council, he'd never seen an exciting proposal presented to that Council, one that was really much bigger than the proposer could safely tackle. The people who live carefully are up in the first decile on the ratings. The people who take on the enemy are down to the tenth decile, or are not rewarded at all.

(3) Leadership and management of holistic approaches. Clearly we need a wholly different notion of management of R & D as we take on our holistic approach.

(4) The need for organizational redesign of the knowledge system if we are going to succeed in the way I'm trying to suggest tonight.

(5) Ways of establishing linkages among science institutions to aid in the developing of a worldwide holism, not just oriented towards American culture and its problems, but also to various cultures and human ways of living across the world.

(6) A recognition of the allies and the enemies of the holistic approach. The enemies have good points in their favor -- because holism run wild could very well destroy the good that we have in scientific institutions today, by a weakening of the disciplinary base.

I have to admit at the end of this talk that I do have some disciplinary biases that can't escape me. I happen to believe that most people who go into planning and helping society ought to know basic mathematics. I think it's the discipline that has provided use with a good language for talking about many holistic viewpoints.

If you haven't caught on, these last six points refer to my interpretation of the tasks that Dick has given us for the six workshops tomorrow. He may be a little surprised to hear my interpretation. This is my version of these tasks. I expect each workshop will succeed in accomplishing these in a grand style.

* * *

DISCUSSION

QUESTION 1 - How do you define the term "science"?

ANSWER - As a philosopher I talk not in terms of its present definition within the institutions of science, but in terms of history. For example, I think Aristotle had a notion of science which is as viable today as it ever was, and so did Kant. But their notions of science would not agree with today's institutional breakdown of science. Now I'm more inclined to take the historical meaning of science which meant knowledge rather than today's breakdown in terms of the institutions of science defined in terms of the disciplines.

There is a "science", for example, of an Indian tribe like the Navahos. They have knowledge of what it is like to live in the Navaho nation that people in so-called scientific communities don't have. I think it is only until we recognize that there is "science" in many different parts of the humanity that we'll come back to my "reactionary" notions, which are Kantian, Aristotilean, or Cartesian.

QUESTION 2 - How was Project Knowledge: 2000, conducted?

ANSWER - Project Knowledge: 2000 is separated into three forums (some reductionist did that). They are: need for, generation and communication of knowledge. How do you separate those three in any sensible way? Of course I don't see how. I said I thought problems were convenient ways in which we could stop the process and look at a segment of the film, but before we put it back together again we want to see the whole thing. As far as the workshops are concerned, it means that if somebody wants to get on with the organizational or the reward system in the conceptualization workshop, I hope the chairman doesn't say, "well, that is being solved by B or C . . . ."

(Richard Scribner) I'd like to add to what Wes has just said. In formulating this conference we've been sensitive, I think, to dilemmas posed by doing just what we've done: when we were designing these workshops we struggled against organizing them exactly in a reductionist mode. We didn't want to do that, but we felt quite uncomfortable with any other way.

QUESTION 3 - Do you think that intuition and feeling belong in science, as well as reason and observation?

ANSWER - The answer is, briefly, "yes." Even in the 19th-century version of science, intuition played an important role. They recognized the need for creative genius that seemed to come out of this mysterious function called intuition. But the 19th century discouraged us from including feeling or evaluation. In the 18th century, Kant regarded his second Critique to be dealing with science as much as his first Critique. The second dealt with ethics and values. The word "science" incidentally, is again a 19th-century invention. I would say that mysticism, mythology, literature, poetry, meditation, all go under the heading of science.

QUESTION 4 - (Gordon Enk) Solving problems today requires working with what you have and can manage. This usually means, at best, working with a limited set of disciplines or areas of professional expertise. Holism is a fine concept or ideal, but won't it simply immobilize anyone who tries to take it as an operating principle into the actual, rational public problem-solving domain?

ANSWER - I really can't respond, Gordon, because I don't know where you are. I was saying that a town board or a county supervisor are involved in a kind of knowledge that the disciplines don't understand well. Since they have to be holistic in their response to issues, and disciplines tend to be reductionist or to divide up the problem into pieces, they are not responsive to public needs. Therefore, we have as much to learn, as scientists, from the town board as the town board has to learn from us.

Gordon, it isn't all that difficult. There's good historical precedent (I'm sorry I'm so reactionary this evening). But, the 17th-century Liebnitz, Descartes, and Spinoza said it as clearly as it could be said: there is only one individual who has holistic knowledge and that is God and none of us has the hubris to be a God. All we can say is that we will strive to come as close as we can.

II.

CONFERENCE PAPERS:

WORKING ON THE PROBLEMS

"While the cure for our energy ills seems to consist of reattaching the disparate parts, the motivation for doing it must arise elsewhere. ... call it what you will, people will have to start giving more to than they take from the problem. ... to try reaching out to touch all parts of the problem, to listen, and to try to contribute to the resolution."

David Rose

THE ENERGY PROBLEM:
A GUIDE TO MORE ADEQUATE APPROACHES

David J. Rose
Massachusetts Institute
of Technology, Cambridge

INTRODUCTION

Energy, food, transportation, health care, and other activities that characterize a civilization all share some similar qualities: tendencies toward reductionism instead of holism, for example. Of these larger problems (in contrast to "small" ones such as offshore oil drilling, which form parts of the larger ensemble), energy is relatively easier to treat than most of the others, because it is a means to an end, and not so closely tied to ends themselves (as food, for example). Though the task is difficult, energy can therefore be analyzed with less social rancour and hopefully more consensus than most other problems. Even so, science and technology are not enough, and this paper, that seems to start with technology, will end on questions of morality. Considering the difficulty with energy, the outlook for solving other problems appears bleak, unless better arrangements are made.

The Problem in Satire

Those unable to contemplate disaster without some touch of whimsy often deserve the predicted fate.

The house is on fire, we tell ourselves; see Figure 1. How do we organize ourselves to put it out? So many dimensions exist: getting water; carrying it to the scene; determining its pH; measuring the latent heat of evaporation; and writing an environmental impact statement. But perhaps things look different when viewed from another dimension; see Figure 2.

So it is with energy. We see the growth sector's view of energy in Figure 3; but extend the time perspective, and see in Figure 4 the limits to growth view. To the growth sector, the year 2000 lies beyond forever, but to the limits to growth sector the year 2020 is the day after tomorrow, and the two groups talk right past each other.

Figure 5 shows what until recently was (and perhaps still is) energy and its effects, as viewed by the automotive

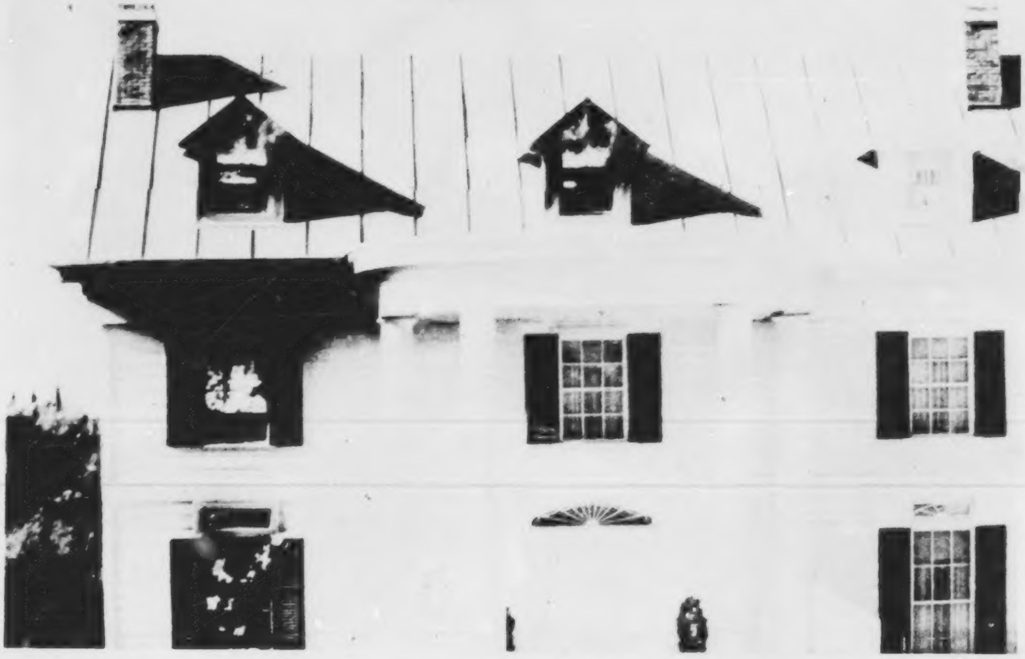

FIGURE 1: CLEARLY, THE HOUSE IS ON FIRE. WE MUST IMMEDIATELY ORGANIZE TO PUT OUT THE FIRE.

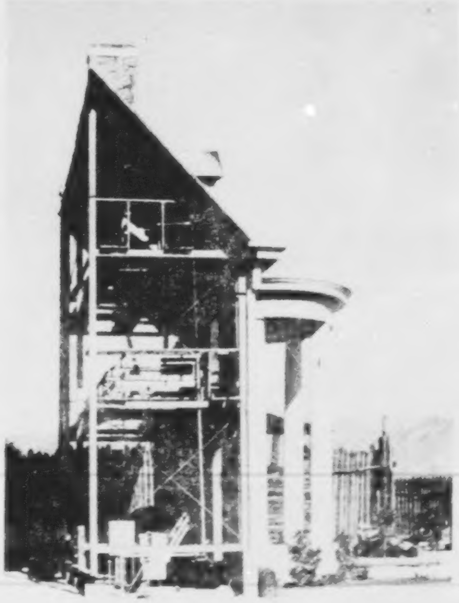

FIGURE 2: THINGS MAY LOOK VERY DIFFERENT WHEN VIEWED FROM ANOTHER PERSPECTIVE.

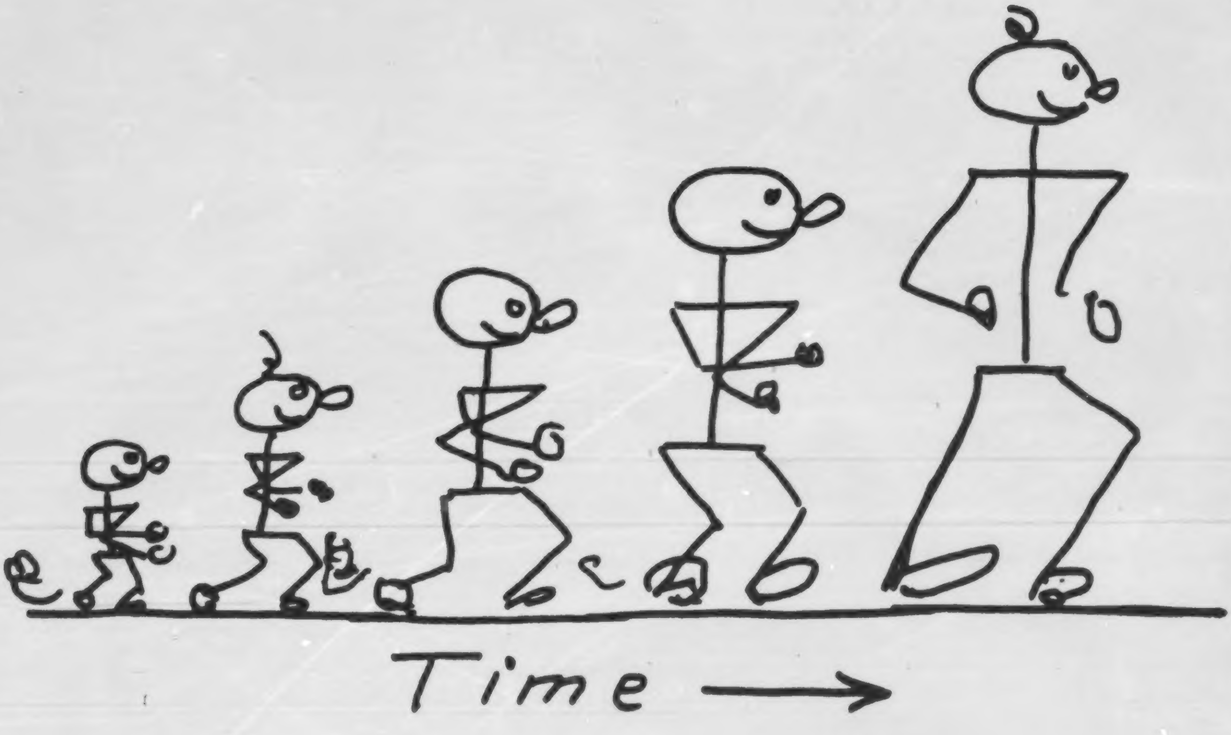

FIGURE 3: GROWTH SECTOR'S VIEW OF ENERGY

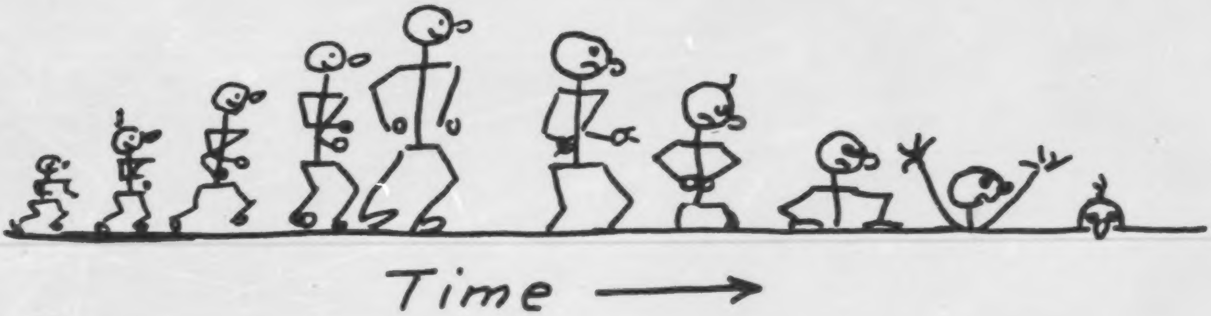

FIGURE 4: LIMITS OF GROWTH VIEW OF ENERGY

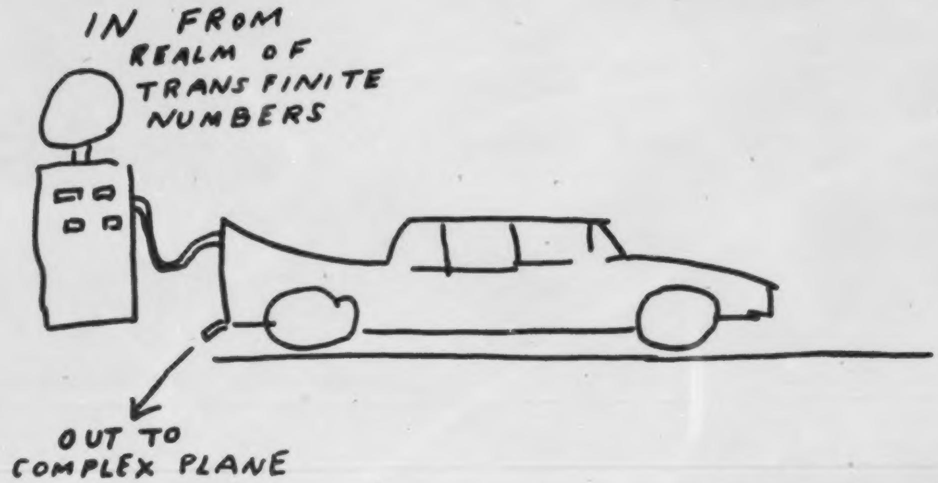
IN FROM
REALM OF
TRANSFINITE
NUMBERS
OUT TO
COMPLEX PLANE

FIGURE 5: AUTOMOTIVE INDUSTRY VIEW OF THE ENERGY PROBLEM

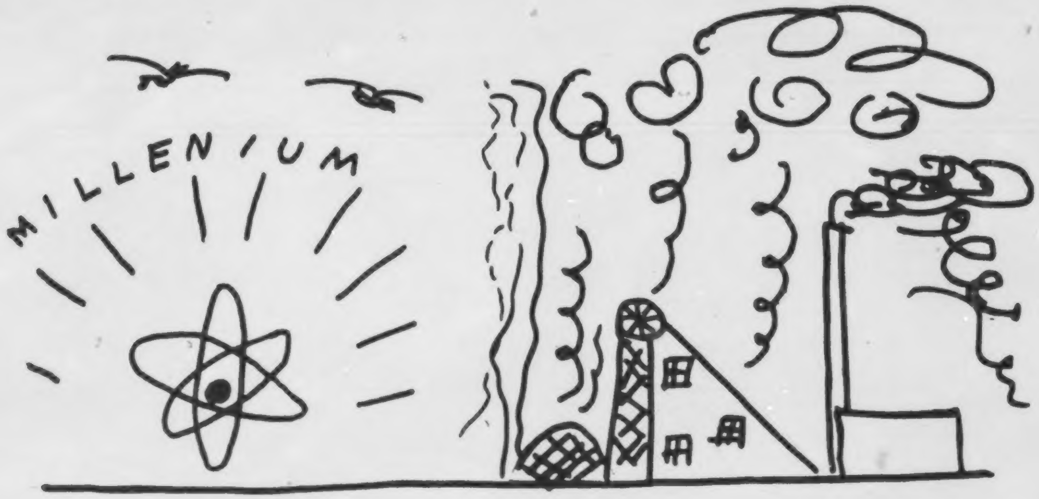

FIGURE 6: NUCLEAR ENTHUSIAST'S VIEW OF THE NUCLEAR VS. FOSSIL FUEL DEBATE

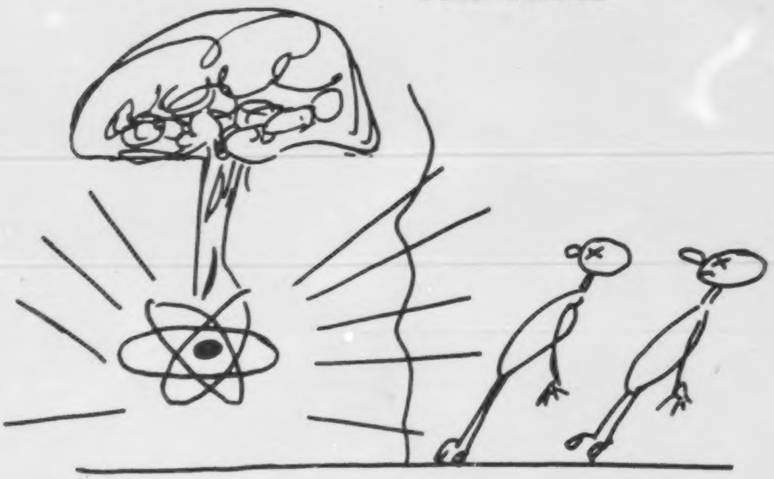

FIGURE 7: ONE POSSIBLE CRITIC'S VIEW OF NUCLEAR POWER

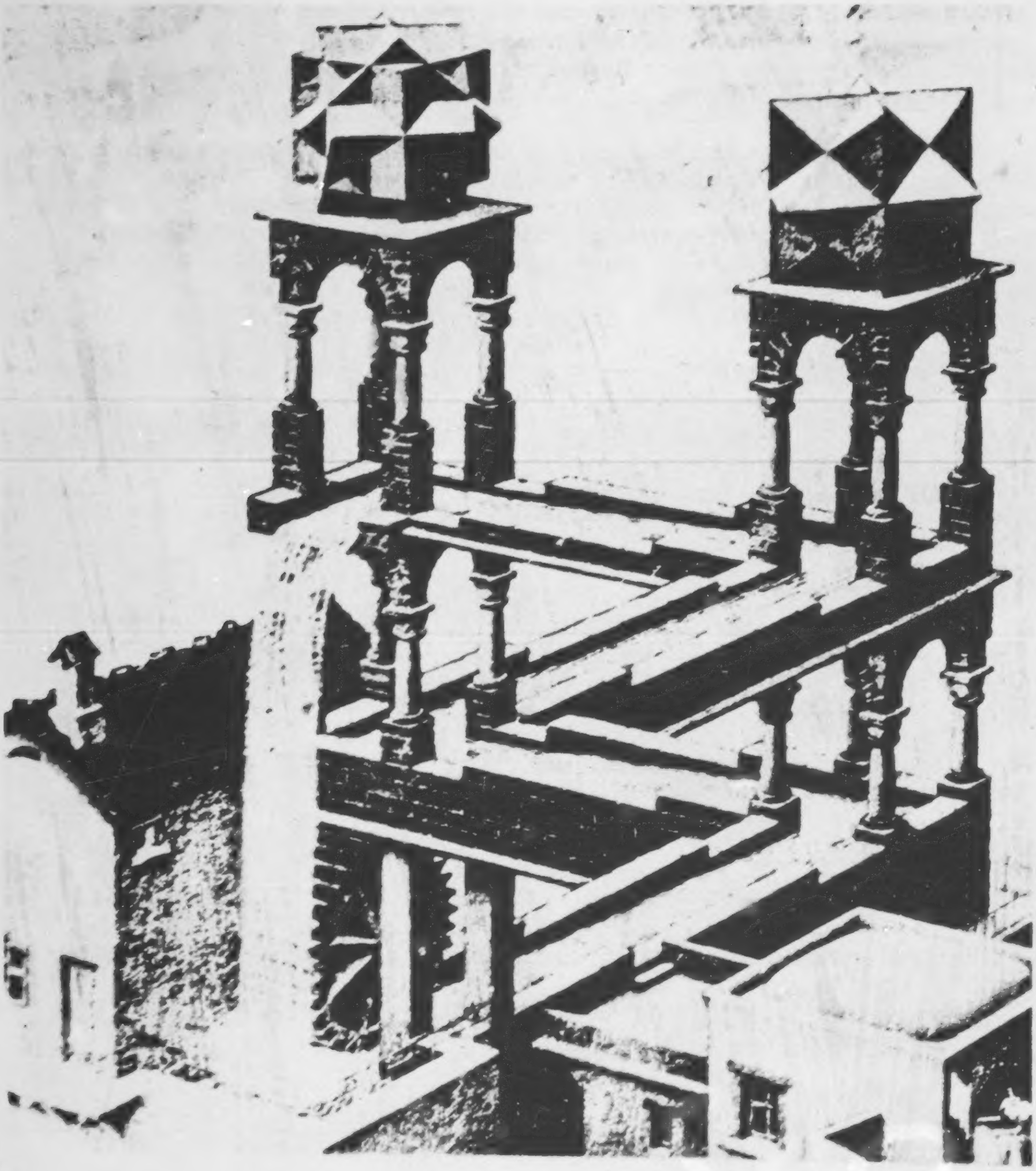

FIGURE 8: CORPS OF ENGINEERS VIEW OF ENERGY

industry, which in the 50's and 60's acclaimed itself to be a paragon of free enterprise, industry expertise, and technological excellence, but found itself incapable of making cars safe, nonpolluting, or even convenient.

Until a few years ago, we had what many described as the Atomic Energy Commission's view of energy - Figure 6; compare the millennium with birds flying in the clear air with the mess on the right side of the figure. Whether it will correspond to the Energy Research and Development's view of energy remains to be seen, but it surely persists with the Atomic Industrial Forum. Figure 7 shows some critics' view of the old AEC and similar organizations.

Figure 8 is self-explanatory, and comes with appreciation of Muarits Escher, of blessed memory, one of the world's great artistic draftsmen. Also Figures 9 and 10 need no editorial amplification; and Figure 11 shows the dilemma of the Office of Management and Budget, who sees money going into a large R & D effort, but cannot see a clear end product.

Nationally and internationally, things are much the same. Were the discussion in France, the word would be energie; and Figure 12 shows how it can be rearranged to show the secret message.

Most of what follows is merely sober amplification of the messages of these cartoons.

Surrogate Policies

Ideally, policy recapitulates the constitutive parts of an issue, pays attention both to broad societal goals and to available means, estimates costs of alternative strategies, finds a consensus, and erects the general framework upon which a category of future decisions can hang. Thus policy is a good place to start.

Dishevelment of U.S. energy policies, decisions, and actions, especially apparent during the period 1973-1975, shows fundamental flaws existing in how these matters were and continue to be perceived.

The Introductory chapter of A National Plan for Energy Research Development and Demonstration: Creating Energy Choices for the Future 1/ sets out the following national goals related to guiding future energy efforts:

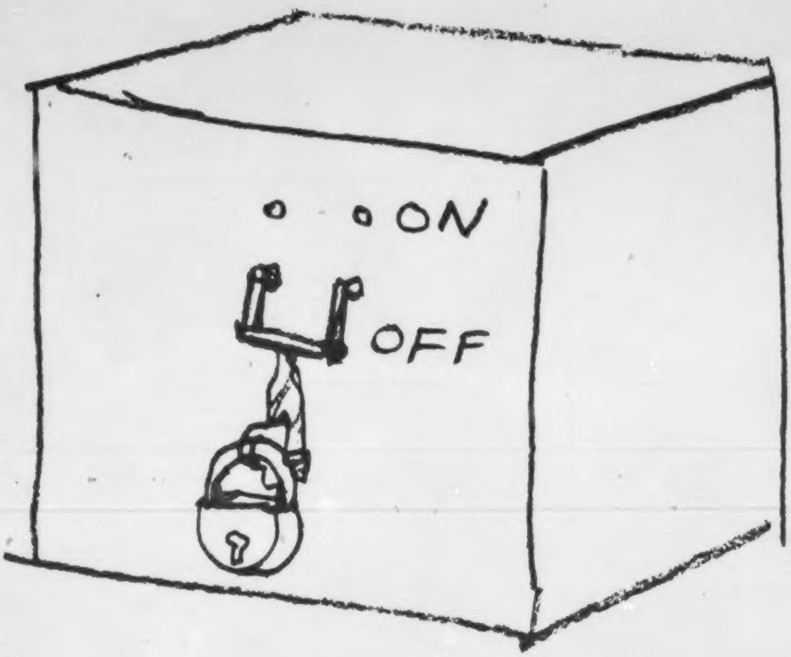

FIGURE 9: SAFETY ENGINEER'S VIEW OF THE NEW ENERGY SYSTEM

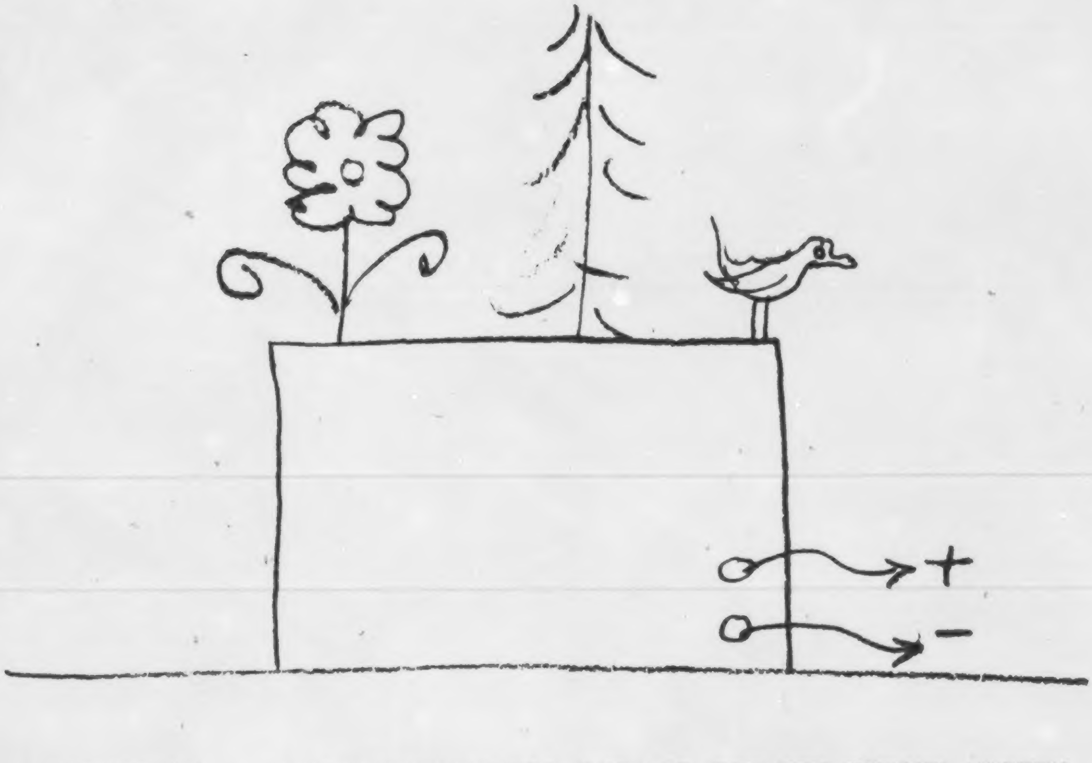

FIGURE 10: ENVIRONMENTALIST'S VIEW OF THE NEW ENERGY SYSTEM

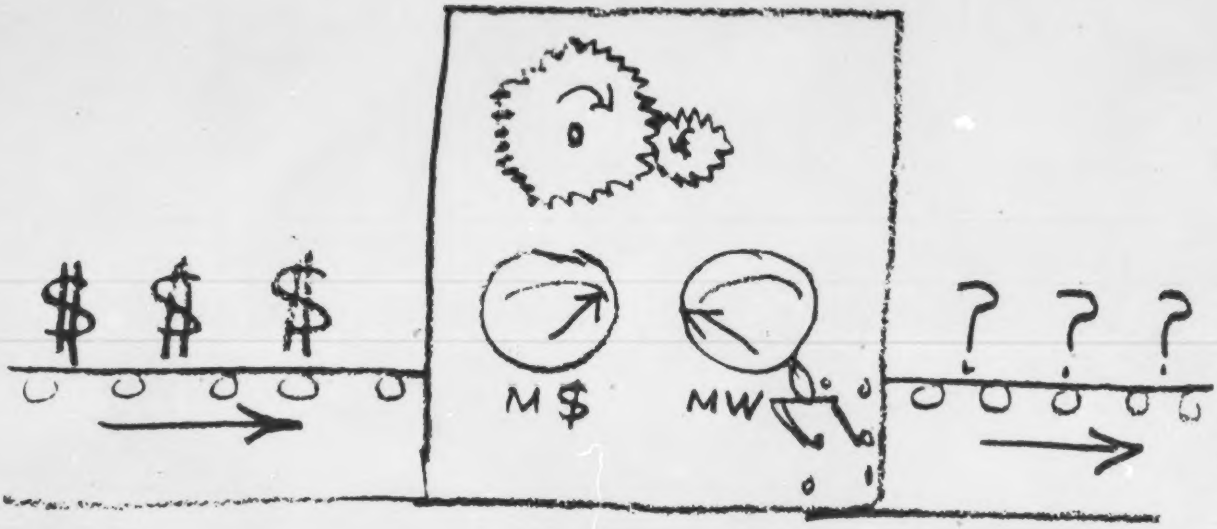

FIGURE 11: OFFICE OF MANAGEMENT AND BUDGET VIEW OF ENERGY R&D

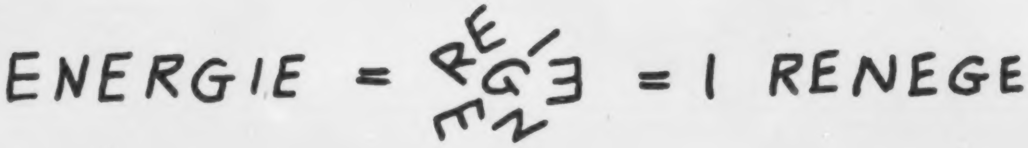
ENERGIE = REGIE NE = I RENEGE

FIGURE 12: INTERNATIONAL DISCUSSION ON ENERGY

1. Maintain the security and policy independence of the nation.

2. Maintain a strong and healthy economy, providing adequate opportunities and allowing fulfillment of economic aspirations (especially in the less affluent parts of the population).

3. Provide for future needs so that future lifestyles remain in a matter of choice and are not limited by the unavailability of energy.

4. Contribute to world stability through cooperative international efforts in the energy sphere.

5. Protect and improve the nation's environmental quality by assuring that preservation of land, water, and air resources is given high priority.

These goals conflict at least in part, which is natural, because many sectors and groups contend for different benefits, on different time scales, in different places, as we have seen. The ERDA goals could in fact form the basis for debating and later choosing rational energy policy, if the inherent conflicts had not been too glibly passed over. The congressional Office of Technology Assessment in its analysis of the ERDA plan, 2/ aims its first overview discussion toward this point:

> ERDA's R, D & D plan, as outlined in ERDA-48, volume I, states five national energy goals to which energy R, D & D should contribute. Heavy emphasis on self-sufficiency as opposed to environmental concerns will have major consequences in the quality of life and economic well-being of the American people. Similarly, emphasizing self-sufficiency rather than international cooperation will have major impacts on our foreign policy. Emphasis among these goals warrants congressional review. Unless there is agreement between the Administration and the Congress on the priorities given different national energy goals, ERDA's development of an R, D & D program is made more difficult.

> A congressional review of the priorities assigned to the five goals takes on particular importance because energy is so central to other policy areas. Other Government agencies will be planning programs ranging from foreign trade to welfare based on their perceptions of these priorities. For these reasons maximum clarification of priorities will be beneficial.

Contrary to the ERDA aspiration, we lived, until 1975, off the dregs of an old de facto energy policy, to wit:

1. Energy should be cheap, almost regardless of the costs to others.

2. Energy could be considered separately from other major societal issues.

3. Energy could be considered chiefly as the business of supplying it.

4. Plenty more energy is around, from the same type of sources we have used before, perhaps at some increased cost, but not to worry.

5. No change in life-style need be discussed, let alone implemented.

6. The energy sector would solve the energy problem pretty well, particularly if left to do it.

All six presumptions are flawed. Energy will cost much more, come from different resources than hitherto, and require accounting for societal costs only dimly perceived today.

Energy cannot be separated from other sectors: from transportation, which uses one-quarter of it all; from industry which uses 41%, from domestic and commercial activities, which use the rest.

There is not plenty more energy around, of the kind upon which the present civilization has been built. Taking petroleum as an example, Table I shows estimates of total eventual world recovery, based on data given by Moody and Geiger. 3/ Of the U.S. supply, about half has already been used, and most of the remainder is predicted to go by A.D. 2000.

TABLE I. Estimates of Total Eventual World Recovery of Petroleum (billions/bbls). Ref. Moody and Geiger. 3/

USA	230	
Canada	85	
USSR-China	500	(?)
Mid-East	630	
Europe	70	
Africa	165	
Latin America	175	
Far East	130	
Elsewhere	30	(?)
TOTAL	2000	

If the world had to depend on this petroleum stock for all its energy, the resource would last about 55 years. Fortunately use of coal and other energy supplies stretches out the petroleum resource; but so does increasing world use tend to contract it.

No way exists to accommodate to the shrinkage of classic oil and gas supplies, or switches in end use, without changes in life-style, because we cannot afford to pay for waste, and should never have countenanced it in the first place.

The energy sector will not solve the problem by itself, because great problems of energy, such as the tradeoff between more energy and decreasing resources, or cheap energy and pollution can only be resolved by including many more sectors in the decision process.

Although all those de facto strategies were false, yet they possessed the overwhelming advantages of reasonable internal consistency, simplicity, and lack of conflict, only provided that the attitudes implicit in them are sufficiently restricted in space, time, and sectorial interest.

The difficulties satirized in section 2 arise from three main causes, not all independent, but easiest to discuss separately. They are: too narrow sectorial interests; incommensurate and inappropriate time horizons; and no sector of any consequence that was interested in the problem as a whole.

Narrow Sectorial Interests

This is the by-now-common topic of excessive reductionism. Rewards come from doing simple specialized things, and creating structures to solve simple subdivided tasks. Examples abound: failure to discuss rationally the nuclear power debate - compared to what? one can ask. But no socially useful answer has come (although I think we begin to see now a more comparative rational debate about options, rather than polemics written for an intellectual vacuum). Many more could be listed: environmental costs, the special importance in choosing properly energy for urban areas, but perhaps what stands out so dramatically is the issue of conservation versus more provision.

Here, energy conservation stands not for the scientific law that energy is conserved, but rather for rational utilization and increased effectiveness. Of nearly 2.4 billion dollars in the Federal FY 76 budget requests, some $33 million was to be for conservation; the Congress raised this to $55 million, still a small amount.

The reason for dominance of energy provision over conservation is basic and simple. Energy provision brings fairly prompt cash rewards to well-developed commercial groups ready to receive them, for doing tasks (drilling, pumping, etc.) that may be technologically complicated, but are fairly simple to organize; many societal costs (pollution, for example) fall upon a generally diffuse and poorly organized group (the public) later in time. On the other hand, energy conservation usually brings later rewards to relatively ill-organized groups; but the associated costs (better home insulation, for example) must usually be paid now. This nonparallelism between provision and conservation limits the latter to cases where the same sector pays the costs as captures the presumably larger benefits. Because the benefiting group tends to be the public at large conservation loses out, unless public law or public spirit dominates.

Examples abound. The capital investment in the U.S. automobile engine plants is between 5 and 10 million dollars. U.S. automobiles consume about 18 billion dollars of imported petroleum per year, figured at the economic margin. Many think that engine redesign could result in 30% petroleum savings. If that be so, the costs of rebuilding the engine factories completely could be recaptured every 1-2 years in fuel savings, after the new engines have displaced the older ones.

But the automobile manufacturers pay the costs, and the public (approximately) captures the benefits. Market pressure works to make such changes, but it is relatively weak; otherwise, the two sectors interact only via the Federal Government, which must therefore play the key role in energy conservation.

The economic benefits of energy conservation are often expressed as the investment required to save on daily barrel of oil versus the investment needed to provide one more daily barrel, the latter being now about $10,000-$50,000. More opportunities exist to save energy at much lower cost, for example, exhaust heat recuperators on furnaces (to heat incoming air) at $1,000-$2,000/daily barrel at most. Much process steam equipment could profitably be replaced by combined electric power-steam installations, using topping or bottoming cycles. The benefits of added home insulation and the shame of most conventional glass-sheathed buildings are too well known to require description here. My own institution, the Massachusetts Institute of Technology, cut its energy consumption by about 25% without particular discomfort; the experience is common.

New technologies for better energy utilization appear; advanced electric-powered heat pumps promise to deliver significantly more space heat in many buildings than would come from the original fuel used at the power station, and burning it in the building itself. Estimates made by many groups, starting with the now defunct Office of Emergency Preparedness, 4/ show that energy conservation amounting to about 30% of previous profligate custom can be achieved without significant change in life-styles. More may be possible, but only after more study.

All this discussion so far ignores the large environmental benefits of energy conservation. The benefits arise not only via resources conserved for later, and simple proportionality of less stripmining, oil spills, etc., but also via even more important subtle ways. More careful and complete combustion of fuel means fewer active pollutants discharged to the atmosphere, and urban air quality (especially) improves substantially. Pyrolysis of urban wastes could provide energy sufficient for only 1-2 percent of total national needs; but the urban waste problem almost disappears, and the waste-handling system can be built to return valuable non-energy materials. The city of Seattle, Washington, is presently starting to install such a system.

Certainly many energy-conserving stratagems will be adopted as energy continues to cost more, as understanding grows of the benefits.

Time Perspectives

Time horizons for considering energy options enter implicitly, explicitly, and essentially. These time horizons depend not just upon the basic problem itself, but also upon one's own role. Most of the business community works on a 14% or greater rate of return on investment before taxes and without inflation; which means that money doubles each five years, and quadruples in ten. Conversely, and important to this discussion, one dollar five years hence is worth fifty cents today, and one dollar ten years hence is worth a quarter now. Thus company time horizons for substantial investment tend to be 5 - 10 years, and even shorter during periods of inflation; neither coincidence nor magic determined that reserves of available minerals, petroleum, etc., were sufficient for about ten years, over decades of increasing use. What happened is that the particular industry kept about ten years ahead in its discovery program. That scheme works, provided more is available at a societally acceptable price increase. But crude oil production peaked out in the

United States in 1970, natural gas appears now in similar difficulties; together the two fuels provide almost 80% of U.S. energy. The resources no longer suffice, and the classic economic strategy fails.

The strategy fails very particularly in respect to development of a rational set of new long-term options. New major options -- coal liquefaction and gasification by environmentally acceptable processes, controlled nuclear fusion, breeder reactors, many long-term conservation activities -- take decades to develop. Many of them will be necessary by the time they are developed; to be sure, not every technological option will be needed; but it being too soon to tell which ones will be needed and which ones not, we need to develop a substantial set of options. On the other hand, the payoff lies beyond the conventional economic time horizon. Therefore,the private sector cannot address the long-term activities with sufficient vigor, and will persist in exhausting present depletable supplies, even knowing that disaster lies ahead by sole concentration on that route. By the time the new options become economically attractive, it is too late to develop them by anything close to an optimal stratagem. We overdrive our headlights.

No Champion for the Cause

No one was minding the shop. Figure 13 shows several energy projections made for the period up to A.D. 2000. Since 1972, these projections have dropped almost monotonically; the Ford Foundation Technical Fix Scenario, ridiculed as absurdly low in 1974 when it first appeared, now appears more nearly as an upper limit.

Recognizing now that energy transcends any state or sectorial interest, the Federal Government has played an increasingly active role. But it was not always thus, and the Federal Government has been a major contributor to the difficulty. In general, its time horizon has been short, and its judgment poor. While the largest fuel resource is coal, the Department of the Interior appeared to tolerate its own Office of Coal Research as a mere token activity until about 1972, with almost no funds, no inspiration, and no plans. That policy would match the wishes of oil companies through the 1960's, who wanted no possible competition from synthetic fuels, until the time came for the oil companies themselves to see the end of cheap petroleum fuels within their short economic time horizons. Now, oil companies have diversified substantially into the coal business. Congressional committees were well-populated from oil-producing states. As some synthetic fuels from coal come within the economic range and time horizon of oil companies, there is clamor for action, and there will be action, as recent Executive Branch statements

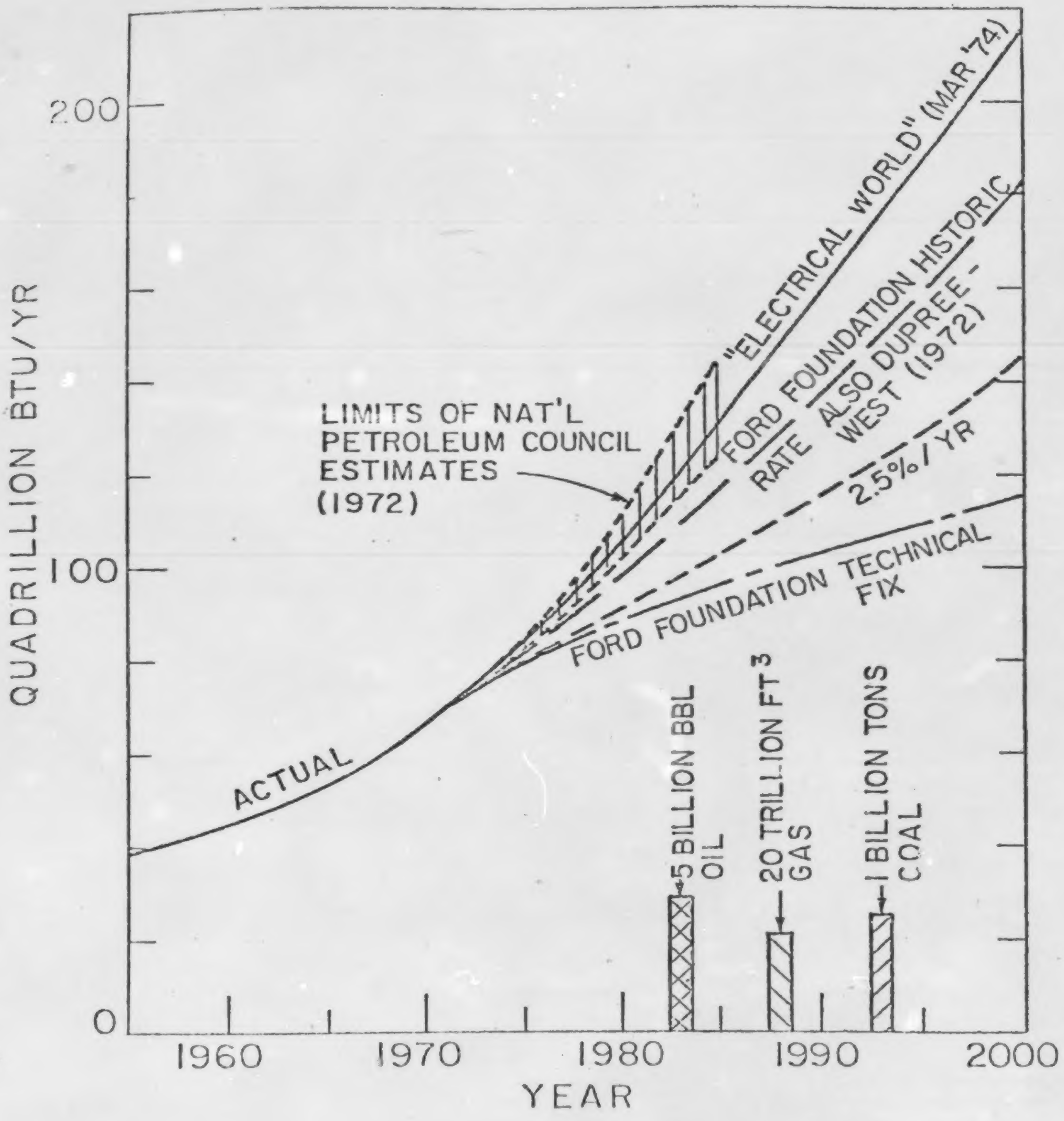

FIGURE 13: SEVERAL ENERGY PROJECTIONS UP TO A.D. 2000. QUADRILLION BTU/YR ARE PLOTTED AGAINST TIME.

show and the ERDA budget demonstrates. Because the time needed to develop the synthetic fuel options in societally safe ways considerably exceeds those short time horizons, the clamor arises for relaxing environmental standards. Unfortunately, those environmental problems are not restricted to land use or emissions standards on pollutants whose effects are understood. For instance, the synergistic effects of sulfur oxides, particulates, and nitrogen oxides are not understood; many new fuels would contain suspected carcinogens or mutagens, and exploration of the possibilities takes time. Much could have been done before, but was not.

Nuclear power, during the past 20 years or more, has remained well-supported, in fact approximately commensurate with its real need, although the program itself could have been better structured. Nuclear energy is useful only for electric power generation (at least at present); thus through the period of its development - the 1950's and 1960's - it appeared as a competitor only of coal, because in those days few environmental standards existed and no one saw petroleum as being extensively used by electric utilities. Oil companies therefore saw no threat, but rather an interesting direction for diversification in the 1970's as cheap oil ran out.

In summary, the Federal Government had, during the initial period before the early 1970's, put only five or ten percent of its high-grade energy research into the fossil fuel area, although those fuels provided about 95% of U.S. energy, and were known to be under the least national control. The policy was one of cheap energy, short time horizons, and an incorrect belief that the private and public sectors had matching needs and motivations; substantial residuals of these ideas remain.

Was There Any Forewarning?

The energy problem (and others) would be excusable if they fell upon us as, say, an invasion from outer space. But it was not so. Warnings, detailed analyses and rational policy suggestions were made available to many sectors. Some were unheeded, some were suppressed. Here follows a brief list, from first-hand experience.

Predictions of finite oil resources had been made continuously and fairly accurately by M. King Hubbard since 1949; he worked for the U.S. Geologic Survey, which was until 1974 in the Department of the Interior. His views being contrary to the conventional (or the desired) wisdom, both the Federal Government and the private petroleum companies largely ignored his work.

As an outgrowth of the attempts both to rejuvenate the National Laboratories and to give them substantially broader roles 5/, a serious attempt was made in 1970 by the Oak Ridge National Laboratory to persuade the Atomic Energy Commission to broaden its role to encompass all aspects of energy: fossil fuels, conservation, environmental and other societal effects, international implications, and so forth. The U.S. AEC was too timid, and also generally thought that a narrow technological structuring of atomic energy was its greatest task. It failed to ask very deeply the two questions: (1) was thinking about energy on a broader scale a proper thing to do? (2) if the answer was yes (as we see now) then because the U.S. AEC had the strongest institutions to deal with many of these matters, who else did they expect would do the work? Such considerations were suppressed until about 1972.

In 1972, the Office of Emergency Preparedness published an excellent (for that period) document, showing how energy savings of about 30% could be made overall, compared with contemporary projections, in gradual stages. The OEP was disbanded.

In 1972, the President's Office of Science and Technology in one of its final acts, set up a detailed review of the energy activities of many federal agencies. The emphasis was strongly on energy provision, especially by petroleum and gas, new coal technology, nuclear fission, nuclear fusion, some ideas about solar power, and a little geothermal power. Rational energy utilization appeared only weakly with respect to transportation and some housing studies. Environmental and most resource limitation topics, plus those with possible political significance, were virtually declared out of bounds. However, the items discussed figures strongly in the $10-billion energy proposal made by Dixy Lee Ray, then Chairman of the U.S. AEC. That shopping list in turn was useful in framing the initial tasks of ERDA in 1974.

By these various inactions and inattention, establishment of a rational U. S. energy policy was delayed at least five years -- probably longer.

Other sectors were no better, and space does not permit listing their misbehavior. For example, the universities, without exception known to me, were followers and not leaders; not one instituted a sizable program until 1971, when already the energy problem had been visualized in ways not much different from how it is today. A main university motivation seemed to be cashing in on A Good Thing, hardly appropriate for putative intellectual leaders.

Where was the Moral Content?

It was all around us. On the one hand, we found general adherance to the view that social good and ready money come by providing energy, in substantial disregard for the facts that not only are resources finite, but also that great benefits accrue via energy conservation. The same imbalance appears in health policy: more concentration on curing the sick than keeping people healthy in the first place, because the first is easier to organize and reward than the second. Any serious reader in the energy field - even the casual magazine reader - can make a list of details.

While the cure for our energy ills seems to consist of re-attaching the disparate parts, the motivation for doing it must arise elsewhere. The motivation is not economic but moral, and the key has been in our hands since before time began. The besetting sin is hubris and the too frequent habit of each group maximizing its benefits, as we so euphemistically or politely put it, or to put the matter more clearly, each group getting all it can.

Then the cure, if that be the fault, is described by another Greek word, agape, for which the Latin is caritas, in Elizabethan times called charity, with a meaning that is now ominously vanishing from the vocabulary. I'm pleased to see one of the later speakers speaking of, mirabile dictu, charitable institutions, presumably not in the sense of eleemosynary ones. Let it be Jewish charity, Christian charity, Socratic charity, or whatever. Call it what you will, people will have to start giving more to than they take from the problem. That is Outreach as my church calls it: to try reaching out to touch all the parts of the problem, to listen, and to try to contribute to the resolution. To be willing to give up something.

The applicability, importance, and distortion of these views in relation to energy appears nowhere better than in the various analyses of nuclear power that have been done lately. Most are bad. The best to date comes, significantly, from The World Council of Churches, who tried earnestly to consider people now, people later, resources here, resources elsewhere, simple hopes of people everywhere, and social justice. They may succeed better than our National Academy of Sciences, who studies these problems at much greater expense, but whose report may flounder in a thousand little particularities. That is why in ten days I go to Geneva to join The World Council of Churches, in the continuation of their work.

FOOTNOTES

1/ U.S. Energy Research and Development Administration, Report ERDA-48, Washington, D.C. (June 1975), obtainable from the U.S. Government Printing Office, Washington, D.C. 20402 ($1.70).

2/ U.S. Congress, Office of Technology Assessment, An Analysis of the ERDA Plan and Program, (October 1975), obtainable from the U.S. Government Printing Office, Washington, D.C. 20402 ($3.85).

3/ Moody, John D. and Geiger, Robert E., *Technology Review* (March/April 1975), vol. 77, pp. 39-45.

4/ Office of Emergency Preparedness, "The Potential for Energy Conservation," (October 1972), A Staff Study.

5/ See Rose, David J., "New Laboratories for Old," *Daedalus* (Summer 1974), pp. 143-156.

* * *

COMMENTARY

Joseph Leary
Energy Research and Development Administration

When I review our present energy status in the U.S., and even in the entire world, I think the outstanding thing that comes to me is that during the past 25 years we could have done a much better job of formulating and implementing an energy policy and an energy program. It is true also that all of the major elements of what we today refer to as an energy crisis were known to most of us in the late 1940's But we have to admit that the broad recognition of the problems and the identification of potential solutions are very recent experiences. It has only been in the last few years that we have really come to focus on this in a national way, and I believe that this has led to a realistic and workable national energy development plan.

Many of the points made by Professor Rose in his paper are certainly true. However, there are some points on which I must take exception. For example, the criticism of federal agencies regarding failure to respond to important issues is too broad. The federal agencies have limitations and they are restricted in what they can do. I think an example of this can be found in the light water nuclear reactor development history. As you well know, 15 years ago the Atomic Energy Commission was heavily engrossed in developing and fostering the commercial development of the light water reactors (LWR). Later they were told to get out of the light water reactor business because this was now an industrial situation and the federal agencies had no business in that field of development. The AEC was told to discontinue the LWR development and,as we see today,the problems were not all solved. Here is a case where the responsible federal agency really had no choice in the course of action. We still have many problems left. And also, in the paper Professor Rose points out that the Atomic Energy Commission should have supported a national laboratory's early suggestion for diversification into energy laboratories. I agree and I think that, in fact, the suggestions coming from Oak Ridge really were early enough to stimulate diversification. But I believe that under the enabling legislation of the Atomic Energy Act that the Atomic Energy Commission was not chartered to support anything other than nuclear energy.

This is precisely why the Energy Research and Development Administration was formed. Admittedly, it was formed years later than it should have been, but at least we are now started.

There is also an implication running through Professor Rose's paper that our industries are insensitive to our national long-range goals, and that specific industries such as the automobile industries and the oil industries are profit-oriented. I believe that this impression is correct, but we believe that this is as it should be. I personally believe that that is a proper function for them. General Motors is not in business to make more efficient, less polluting cars. They are not even in business to make cars. They are in business to make money and to make it on a short-term basis. Clearly the long-range, high-cost energy problems have to be solved by something other than industry. They are not there to do that. That has to be done somewhere in the public sector, somewhere in the Federal Government, presumably through that awful thing called bureaucracy. At least I don't see any other way of doing it.

Now, in conclusion of this part of my commentary I would like to say that the title of the paper by Professor Rose, namely, "The Energy Problem: Fragmented Resource - Specific Approaches Don't Work" is misleading. I believe that resource specific approaches will work, but not overnight. They won't solve the overall, moralistic questions you raise by any means, but I think our first goal is similar to the old recipe to make a rabbit stew. First, you catch the rabbit. First, we must have specific energy sources to develop. I think that is where we are starting.

The basic plan of ERDA, which I don't think all of you are familiar with, has been to divide the approach into three chronological phases: the near-term, the midterm, and the long range. By long range we are talking of the era that begins in the year 2000 which is only 25 years away.

Getting into the three phases or three terms of the ERDA program, the near term is obviously to press major conservation efforts that reduce energy consumption and shift the consumption to nonpetroleum sources. Conservation is going to be of great help, but a number of studies have emphasized that energy conservation alone will not solve the problem. There is no question that great benefits can be gained from conservation.

A second part of the near-term program is to establish those technologies that will permit an immediate expansion of existing principle energy sources: oil, coal, gas uranium; by direct utilization of coal by industries and utilities; by nuclear converter reactors; and, through enhanced recovery of oil and gas.

The midterm is really a limited area. There are only two major additional technologies identified; gaseous and liquid fuels from coal and oil shale. In addition, many other marginal things are being pursued such as geothermal, solar heating and cooling, energy storage, etc. You know all of these.

Each of the three long-term options has technical, environmental, and cost uncertainties. Thus, because of the critical need for success and the uncertainty of solutions for the problems, all three technologies are being pursued vigorously today. They are high cost options but they are cost effective.

There is very little time left to solve the major problems. Unfortunately, we have thrown away our opportunities during the last quarter of a century. We now have to play a difficult catch-up game. There is no room for any major error. We will all have to suffer through minor errors I'm sure.

In summary, I believe the energy problem and the solutions are being addressed adequately by the Federal Government, but this is just starting. We now understand our problems better and we understand means to alleviate them. These means are being emplaced. Obviously, the approach and the implementation that is being used now can be improved and must be improved. The federal agencies need your help. Through conferences such as this you can clarify your ideas. But, you must make a strong effort to teach the federal agencies the virtues of the approaches that you are developing. As emphasized in preprints for this conference, one of the anticipated outcomes of the conference is action regarding the enhancement of the environment for interprofessional, public, problem-oriented collaboration; another means of more effectively coupling science and engineering to social needs.

* * *

DISCUSSION

David Rose: I'd like to make a couple of points. First, one that we can easily dismiss as ancient history that won't happen again. But, from being on the President's Office of Science and Technology (OST) (of blessed memory of 1972), on the group that was supposed to look into energy strategies for the United States, and being on tolerably good terms with the man who drafted the first presidential energy message and knowing that the OST work, energy conservation was a dirty word and one was not allowed to ask where gas came from -- it came from an infinite hole in the ground -- which tempts one to make some scatological comments. And, in the second case knowing that the first draft had a lot in it about conservation and it was XXed out by Mr. Erlichman with various remarks about "this isn't the ethic," one can easily say that we have passed those points and they will not return. But the question before us is not just that.

It is harder to see the future. It is hard enough to see the past. But let me give you a new scenario for the future. How will be avoid this one? Most people live near cities and it is even the statistical truth that the highest density of people is in cities. Also, the highest energy use per unit area is in the region of cities so the largest impact of energy policy will fall on cities in terms of either environmental impact which can be either positive or negative, and many other effects too -- on transportation, on building design, and so on. And who is thinking of the problem of energies and cities which is going to be one that is going to be along in a couple of years. In fact, I declare it to be here. And, who do we find working on that? You knock on the doors and there is no answer.

How do we make institutions that will see ahead and do not overdrive their headlights? This is the question. It is a worrisome question. ERDA is doing vastly better than the old organization. I agree with you. Things are very much better. But, have we gotten there yet? What more do we need to do and what needs to be added? Are we again looking for gods in machines, which are none.

Ann Macaluso: The thing that troubles me about this discussion so far is that I have that feeling that ERDA is being tabbed with a responsibility which I'm not sure it ever was expected to have or ever can have. That argument really is not over the fact that it sets priorities among energy resources, but that it has set its priorities in the wrong places. I bet everybody in this room would probably much prefer to have those resources placed on conservation and solar energy which is much sexier than nuclear fission or nuclear fusion, and probably more efficient.

The real question it seems to me in this whole energy issue is not energy policy so much, although that certainly is an issue because policy is a function not of a single agency or of ten government agencies, but of the citizenry and the professional capacity in the country and the leadership community and the Congress and all the institutuions that exist. If we are really and truly to look at energy as the primary aspect of what might be called the more holistic approach it seems to me we do have to deal with a great many more of the issues that are cross cutting and that are of vital concern to each of us as individuals in our own heads; and of vital concern to each of us as individuals who contribute in some small degree to making up what turns out to be the way this country functions. I don't know whether that makes any sense, but it does seem to be there is a meeting point between these two perspectives and that it is not really fair to fault ERDA for doing everything that all of us have to do.

David Rose: Let me comment here a minute. I think this meeting is already a splendid example of what we face. We mention energy and everybody says, "ah, energy." Then they start to talk about this and that and all the little bits. We might as well have chosen education.

I'm in an educational institution and they stink. There are all kinds of problems. My own institution, under the guise of public service, worries about its overhead because it wants to meet a payroll and pay me, rightly or wrongly; but worries more about looking good and forming a client/patron relationship which can end up by being a distortion of the fact.

One tends to dig down into whatever there is and get lost in the details instead of looking at the whole picture. History is full of that kind of thing. If one reads back into ancient English history, one finds in the 16th century they had a renaissance of knowledge which led to over-education -- it also led to the revolution which also led to the cancellation of the schools and so on.

Joseph Leary: I think that the analogy with the universities is a very interesting one. I think, for example, the criticism that we have our national resources generally arranged on the wrong priority system, is not limited to ERDA. But the universities also, I think, have problems with many of their priorities. Identification of their resources has been wrong, just as you are saying.

It is a common problem. Downgrading it again to an energy talk, my own feeling is, the major energy problem is the fact that no one is aware that there is an energy problem except for select groups like this. The average person doesn't believe there is an energy problem. He doesn't believe an energy problem is imminent. To me that is the major energy problem we face.

" ... the (National Institute of Mental Health's) mandate for research on social problems is significantly broader than its mandate for operating programs, so that the development of organizational mechanisms for linking the two becomes a critical issue."

Ann Maney

"There is ... a critique ... that these large social experiments ... (are) wasteful. The alternatives seem more wasteful."

Clark Abt

THE NIMH EXPERIENCE IN SOCIAL PROBLEM RESEARCH:

INSTITUTIONAL CONSTRAINTS ON HOLISM

Ann C. Maney
Mental Health Study Center
National Institute of Mental Health

This presentation is an abbreviated version of a longer paper.* It draws heavily from Chapter 11, "Research on Social Problems" (co-authored by Melvin Kohn, Ann Maney, Saleem Shah, Leonard Perlin, James Goodman, and Marguerite Young) appearing in *Research in the Service of Mental Health: Report of the Research Task Force of the National Institute of Mental Health*, edited by Julius Siegel *et al.* 1/

PREFACE: THE NIMH RESEARCH TASK FORCE

The National Institute of Mental Health (NIMH) Research Task Force was constituted to study the Institute's research activities and to make substantive and organizational recommendations to the Director for future directions. Areas for attention were divided among 10 study groups which largely reflected the existing NIMH organization. As a result, the domain of the Social Problems Study Group excluded mental illness and behavior disorders, drug abuse, and alcoholism; issues in application and utilization also were assigned to other study groups, as were underlying social processes. The extent to which these restrictions shaped the nature of the Social Problems Study Group's procedures and recommendations, and the selection of material that went into the Task Force report was, and continues to be, the subject of considerable controversy among the Study Group members.

The process of organizing the work of the Task Force did take account, however, of the intended role of the NIMH in the development of science, its interrelationship with the scientific community at large, and its own decentralized authority structure.

The charge from the Director included questions such as these:

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* The original paper, which should be the source for all references and quotations, is available on request from the author.

- What progress has been made in each of the many scientific fields involved in mental health research? What important questions remain unresolved?

- In the years ahead what research investments are likely to be the most profitable -- both through increased basic knowledge of the biological, psychological, and social mechanisms influencing human behavior and through improved methods of treating and preventing specific mental health problems?

- What research activities, though promising, now carry a lesser sense of urgency and opportunity?

- Among the many administrative and organizational support mechanisms that have accompanied the rapid growth and diversification of the Institute's research programs, which ones are still serviceable and which need to be modified or replaced. 2/

Initially, the study groups were made up largely of research scientists from within NIMH. They were autonomous in that they chose their own chairpersons and set about their task as they saw fit. Ultimately, about a third of their membership was drawn from the outside, with consultants numbering more than twice their membership assisting in various ways.

In part then, the process of organizing the work of the Task Force was a model of participatory management in a traditional scientific enterprise with intramural and extramural components. In the social problems area, however, other parts of the process resulted in the kind of conceptual fragmentation that is the concern of this conference. What follows presents reflections of this fragmentation in recent NIMH-sponsored social problems research, examines its historical roots in organizational structure, and recommends organizational strategies for fostering holism given the constraints of the current structure.

THE ROLE OF NIMH IN SOCIAL PROBLEMS RESEARCH

In 1955 legislation authorizing the expenditure of funds to support a nationwide study of mental health problems by the Joint Commission on Mental Illness and Health, the Congress indicated which mental health areas required greater attention and study. Concern was expressed over the great number of mentally ill and retarded hospital patients in the country, the outmoded reliance on custodial care in mental hospitals as the chief method of dealing with mental illness, the great lag between the discovery of new knowledge in the mental health area and the practical application of such findings, and the extent to which it appeared that many emotionally disturbed children were being placed in mental hospitals without appropriate treatment facilities. At the same time, the Congress also identified a number of social problems as being of special mental health concern: alcoholism, drug addiction, juvenile delinquency, broken homes, school failures, suicide, absenteeism and job maladjustment in industry, etc. The act was passed as a Joint Resolution of Congress without a dissenting vote.

The next major legislative milestone occurred in 1963 with the Community Mental Health Centers Act, which emphasized the growing concern of Congress with problems of alcoholism and drug addiction. In budget hearings that year, the Congressional Committee urged that NIMH foster "imaginative approaches to such difficult problems as alcoholism, delinquency, and drug addiction." The Institute's program expanded quickly in these problem areas as well as in the areas of school mental health and suicide.

In 1966, the Institute organized a symposium to help define its responsibility for the social problems area. A major recommendation was that the National Institute of Mental Health focus its research activities on selected social problems. The list went beyond the congressional specifics. It included not only such explicit congressional concerns as mental retardation and crime and delinquency but also aging, minority problems, metropolitan problems, poverty, disenfranchised groups, homosexuality, marital discord, mass violence, and housing and related issues.

The scope of the Institute's concern with social problems had expanded far beyond the fate of the hospitalized mentally ill to include the study of underlying social conditions that have a strong potential for producing distress and some form of markedly diminished psychological or social functioning, regardless of whether such dysfunction also involves a severe mental illness.

The administrative response, on the other hand, has progressively narrowed its focus to popularly recognized categories of social problems. In the Institute's early years, research and development programs did address social and psychological conditions underlying social problems. Then increasing numbers of extramural grants were organized around a combination of specific and catchall problem areas and problem services, culminating in the organization of Centers focused around specific problems.

Although two of these Centers -- the Center for Studies of Metropolitan Problems and the Center for Minority Group Mental Health Programs -- were named in terms of demographic categories pointing to underlying social conditions, the activities of other Centers which singled out discrete deviant behaviors -- alcoholism, drug abuse, suicide, crime, and delinquency -- were not conceptually coordinated at any point. What's more, this categorical mode of organization later was extended beyond the Centers to the Division and Institute level with the elevation in status of multifaceted programs in mental health services, alcoholism,and drug addiction. In fact, it has been activities around the three problems excluded from the domain of the Social Problems Study Group that were most completely restructured and funded to bring about "targeted" programs, and they have been the areas of most rapid organizational growth. The result, paradoxically enough, has been the achievement of increased coordination within particular problem areas at the cost of a progressively fragmented organizational approach to the underlying processes.

The fragmentation of the Institute's organization for research in social problems is not altogether attributable to the kinds of targets selected in developing "targeted" research, however. From the earliest days of NIMH, whatever resources have been specifically available to social problems research have been divided very deliberately between two organizational strategies: one geared to supporting the best research initiated out of the state of the science by individual investigators; the other geared to developing critical pieces of knowledge essential to advancing problem solutions by setting priorities and by creating special programs. The organizational units performing these functions and the balance in allocation of resources have changed over time as a problem-oriented mode of administrative organization has taken hold, but the dual strategies have been continued.

A systematic review of new projects in fiscal years '68, '70, and '72 for relevance to social problems 3/ indicated that as much new work was being generated outside of the two programs 4/ carrying formal responsibility for social problems research as was being generated within.

This NIMH-supported social problems research was being administered as "basic" social science research in mental health -- intramural and extramural -- and as mental health services research -- intramural and extramural. It did not flow from a program concern with social problems research per se. A focus on social problem commonalities and underlying processes appears in mental health and mental health services research activities.

When the money allocated to new social problems work was examined as a proportion of total program budget, the most viable program interest was concentrated in the organizational units designated to administer extramural grants for social problems research, laissez-faire and targeted. Outside of these units, the most viable program interest occurred in two intramural research units which were charged with carrying out "basic" social science research in mental health and mental health services.

AN ASSESSMENT OF THE NIMH EFFORT IN SOCIAL PROBLEMS RESEARCH

Review of Projects

One perspective on NIMH support of research in social problems is afforded by a systematic examination of the research projects actually supported in three recent fiscal years -- 1968, 1970, and 1972. This analysis is limited to research in or directly pertinent to social problems other than mental disorders, alcoholism, and drug abuse.

A simple statistical analysis of the main objectives of these research projects indicates that their principal thrust has been treatment and amelioration. Nearly half of all the studies fell into that category. A secondary emphasis has been on descriptive studies of the problems themselves or of their (negative) consequences. Only a small proportion has been addressed to studies of causes and underlying conditions or theoretical or methodological issues, although a slight but significant increase in this area took place over the five-year span treated.

The descriptive research that NIMH has supported on the phenomenology of various social problems and on their consequences has been impressive for its richness. But this research has been largely focused on the full-blown manifestations of particular social problems, mainly on their psychological consequences for individuals. Further analysis of the research on amelioration showed that much of it also had been based, explicitly or implicitly, on the belief that the problems are essentially individual and psychological; remedy is sought in some form of therapy for the affected individuals. Evaluation of remedial efforts, when it occurred, reflected the perspectives of the program initiators -- decision-makers or researchers -- and omitted that of less powerful interested parties as well as that of disinterested outsiders.

In short, NIMH-supported research on social problems not only favored study of the individual and emphasized treatment and amelioration, but also slighted rigorous research design and systematic evaluation. On the other hand, some part of the NIMH support for social problems research in recent years went to studies of problem commonalities and to studies of causal conditions. Some amelioration efforts were formulated around underlying social conditions or around the restructuring of service delivery systems. Some studies of amelioration and change included objective evaluation mechanisms, and a very few involved interested parties in the change effort and in the evaluations. Altogether, there has been a small but increasing amount of support for differently conceptualized social problems research: more social and more pluralistic in design.

Any effort to foster these emergent signs of a more complex model of social problems research must take account of the fact that the Institute's social problems research effort does reflect organizational imperatives, however. The focus on individual treatment, for instance, is stronger in the organizational units concerned with investigator-initiated extramural research on services, both mental health- and social problem-oriented. 5/ Discipline-oriented organizational units, 6/ intramural and extramural, generated most of the studies exploring the phenomenology of social problems and their consequences, work which is also largely focused around individual psychological processes although there is an accompanying concern with underlying social conditions. It is the Special Mental Health Program Centers, 7/ organized with multiple functions around categorical problems, that are generating a major share of what little research is focused around issues of cause, theory, or method.

Review of Utilization

Another perspective on the NIMH research effort emerges when associated efforts in the diffusion and utilization of the knowledge produced are examined. As a rule, research findings were disseminated in scientific publications by the investigators who conducted the research. These efforts clearly relate to the values of science and the scientific community; findings are appropriately exposed to the scrutiny of peers and added to the existing fund of knowledge. Such dissemination of research findings is important, but it does not make knowledge quickly or systematically available to potential users in program agencies, in policy-making bodies, or in the public at large.

In 1972, public information efforts at NIMH were being undertaken by or coordinated through the National Clearing-house for Mental Health Information and the Office of Communication, but there was little systematic effort specific to social problems. Although the development of administrative units concerned with research on services might have been designed to couple scientific efforts to operational applications, our earlier review of projects from three recent fiscal years strongly suggests that the organizational units administering social problems services research were not conceived in this fashion. Nor were there many signs of research concerned with identifying means of overcoming political, social, or organizational barriers to the utilization of new knowledge and technology.

Some exceptions were notable. The Center for Crime and Delinquency was funding an unusual number of remedial projects along with analytical studies and encouraging investigators to undertake special user-oriented information dissemination and research utilization. The Mental Health Services Development branch was utilizing empirically derived 8/ predictors of research utilization in evaluating proposals for research on problems in the delivery of mental health services. This branch also was active in retrieval of existing knowledge needed to improve and change mental health services: a five-volume series entitled *Planning for Creative Change in Mental Health Services* was published in 1971; *Innovations* magazine was developed to spread knowledge about services improvement among potential users; and *Evaluation* magazine was developed to communicate scientific methods in addressing and improving mental health services programs to local users. Efforts in application and utilization seemed to be rudimentary altogether, but more advanced in the mental health than in the social problems area.

Review of Programs

Yet, a distinctly different perspective on NIMH's past record and future potential is afforded by a close inspection of the core programs. They have been small in scale, undersupported in staff, funds, and, in some cases, administration, but they have nevertheless had some important accomplishments. Judged against the field of social problems research as a whole, or even against the Institute's own programs in other research areas, these programs have been miniscule. Their importance lies in their demonstration that where the Institute has tried to develop even modest programs of thoughtfully planned research in areas of direct pertinence to social problems, it has succeeded.

A. Special Program Centers -- One model of NIMH endeavor in social problems research, which could serve as a basis for future development, is the problem-oriented center.

Such centers are devoted to a broadly based program of intramural and extramural research, and to the application of research results to some particular social problem or cluster of problems. These centers (which comprise the Division of Special Mental Health Programs) are relatively new, and none of those currently extant has been fully staffed or adequately funded. None has yet been able to mount its intended program of intramural research. None has yet been able to devote the staff time necessary to fully formulate its program. Yet they have taken hold. From the summaries of social problems research projects, supported by the Institute, one can easily differentiate the areas in which NIMH has active problem-oriented centers from those in which it does not. In the former, the projects are often impressive, their sum total is more scatter-shot.

Among the best organized of the centers are those concerned with crime and delinquency, urban problems, and minorities. Since the Center for Studies of Crime and Delinquency is furthest advanced in formulating and instituting its program, it can serve as a prototype.

This Center's program is marked by comprehensiveness in two senses. It is comprehensive in its explicit recognition that a sensible program of research in this area has to be concerned with more than basic research on the biology, psychology, and sociology of aggression. It also has been concerned with methodological research on indices of crime and criminal behavior, with research on the social structural conditions conducive to criminality, on the interrelationship of biological, psychological, and social conditions in the genesis of various types of criminal behavior, on the functioning of the criminal justice system, and on the effectiveness of prisons and of treatment and rehabilitation systems. It is also comprehensive in its explicit recognition that research and research utilization must be coordinated, and that part of the responsibility for this coordination rests with those who do research and those who support it. Although still lacking an intramural program, the Center provides a model of the usefulness of the Institute's taking the initiative in formulating and sponsoring a coherent program of research and research-related activities with respect to an important social problem.

B. Mental Health Study Center -- A second model of NIMH-supported research into social problems is provided by the Mental Health Study Center in the Division of Mental Health Service Programs. Many lessons can be learned from the long history of this Center, not the least of which is that it is wasteful to move a research group from Division to Division and to change its assigned mission repeatedly over the years. Yet, despite the organizational buffeting that this Center has endured, it has proved the usefulness of having a multiproblem program located in the field, where it is not restricted to a single social problem or to one discipline. It emphasizes a multidimensional concern with social problems as they emerge, as they are experienced, and as they are dealt with by some local community.

Some of the research emanating from this program has focused on discrete problems, such as adolescent runaways, high school dropouts, institutionalized children, delinquents, street corner men, and children with reading disabilities. Other research as focused on conditions antecedent to the problems (e.g., peer influences on black adolescents, test anxiety in elementary school children, prejudice, the relationship between residence and community satisfaction) or on organizational contingencies of remedial action (e.g. police roles, bureaucratic solutions to professional problems of public health nurses, etc.). On occasion, second generation projects employing the findings of studies have developed social action or social change research. These demonstrations have been rare and when they have occurred, have been organized around the restructuring of services in some community institution rather than around basic social structure.

C. Laboratory of Socio-environmental Studies -- A third model for useful NIMH support of social problems research is exemplified by the Laboratory of Socio-environmental Studies, in the Intramural Research Program. Although relatively little research on social problems is done in this program, this little is enough to demonstrate that the working conditions for science so well-established in the Intramural program are as conducive to good research on social problems as they are to good research on, say, biochemistry.

One series of studies that is particularly pertinent to social problems, because it focuses directly on the psychological impact of larger social structure, is the research on the psychological effects of social class position. Begun over 20 years ago in research designed to untangle the interrelationships among social class, family process, and schizophrenia, these studies have gone to a systematic exploration of the reasons why social class affects not only the incidence and prevalence of schizophrenia but also psychological functioning in general. The research has been noteworthy for its efforts to go beyond establishing correlations between class and psychological functioning, to examine the systematic differences in life conditions that account for these correlations. In particular, the research has delineated class differences in occupational conditions that play an important part in molding class differences in conceptions of reality -- in self-conception, social orientation, and even in intellectual functioning.

ON THE APPROPRIATENESS OF NIMH RESEARCH IN THE AREA OF SOCIAL PROBLEMS

Except in the fields of mental disorder, alcoholism, and drug abuse, NIMH has not been a dominant or even major force in social problems research. In other social problems areas, NIMH has from time to time supported significant work but it has not maintained sizable programs over long enough periods of time to have been of major importance in shaping the fields. The Institute has done enough, though, to demonstrate its capacity to support and conduct fundamental work in such diverse fields as crime and delinquency, urban problems, discrimination, and poverty. And it has supported a few major studies of considerable conceptual relevance for the field of social problems research as a whole.

It is also clear, however, that there are several serious deficits in most current research on social problems that also hold true for NIMH-supported work: an underemphasis on underlying and predisposing conditions, particularly social conditions; a lack of attention to the probability of social structural commonalities in the causes and consequences of social problems; an underemphasis on the interplay of social structural, psychological, and biological factors; an underemphasis on processes of change; a neglect of those issues of value and power which affect the development and the labeling of problems and the remedies pursued; and a more than occasional lack of rigor in the evaluation of the effectiveness of ameliorative programs. Work that is substantively distinctive or at the leading edge of social problems research emphasize these assumptions.

- That social problems are not merely the expression of individual psychopathology.
- That the definition of what constitutes a social problem is fundamentally social and the definitional process itself warrants investigation.
- That specific problem behaviors stem from the interplay of social, psychological, and biological conditions.

- That many social problems are interrelated and that what we learn about one problem may have important implications for others.

- That both research on the underlying causes of social problems and research on amelioration of the consequences of such problems are necessary.

The NIMH, in its role as a specialized research institution, is better equipped to sponsor and conduct some types of research than others. Given the assumption of interrelatedness of social problems in both their causes and their consequences, it would be foolhardy to draw up a list of problems that fall within the Institute's proper purview and another list that fall outside the Institute's appropriate area of concern. However, the Institute's focus, in line with its legislative history and its very name, has been on problems that have major psychic or behavioral implications. This is a focus defined in terms of dependent variables, implicit or explicit; it says nothing about independent variables, which may be biological, psychological, social, or any combination thereof; nor does it speak to issues of reciprocal causation. The Institute also should and has been building on its strengths. NIMH has the capacity at present -- and even more in potential -- for supporting and conducting a broad spectrum of social problems research in the biological, psychological, and some of the social sciences. In pursuing these directions, however, the Institute's mandate for research on social problems is significantly broader than its mandate for operating programs, so that the development of organizational mechanisms for linking the two becomes a critical issue.

ON ORGANIZATIONAL STRATEGIES FOR NIMH RESEARCH IN THE AREA OF SOCIAL PROBLEMS

As various social problems have come into prominence, organizational entities have been created within NIMH to do research on each of them. Where once a single branch funded all extramural research on social problems, eventually each problem acquired a branch, a committee, a center, or even an institute of its own. Moreover, these various entities have been established within, or assigned to, or reassigned to, a number of larger divisions, so that even before the reorganization of NIMH into ADAMHA, social problems research was being supported in several separate divisions, with no person short of the Institute Director himself having overall responsibility for their coordination.

To some extent this "fragmentation" of the overall program of social problems research has been the price paid for coordinating within a single organizational unit (the Special Mental Health Program Centers) all research and non-research activities relevant to some particular social problem: it has been the price paid for a strategy selected to make research more relevant to social need. The fragmentation also results, in part, from the deliberate decision to support some social problems research through problem-focused organizational entities, some in direct competition with discipline-oriented research and some intramurally. Such organization brings a gain in quality that comes from a diversity of approaches, but not without concomitant losses.

The Institute's organizational history with respect to social problems research also has been marked by an extraordinary amount of flux and change. Organizational entities have sprung up with bewildering frequency: programs have been reassigned repeatedly; the support of programs has not always been consistent with the enthusiasm that bore them. The Study Group was confronted with the challenge of recommending organizational forms that would provide a continuity of effort regardless of the particular category of problem at the forefront of public attention at any particular time, and to do this while recognizing both the important role of science and scientists in conceptualizing social problems research and the essential fact that the ultimate goal is the solution of societal problems.

In our organizational recommendations, we tried to address this fragmentation and discontinuity while at the same time maximizing the benefits of the current organization. Our recommendations, briefly summarized, were these:

- Adapt organizational devices for broadening the sectors of society represented in stimulating, approving, and administering the Institute's research programs on social problems.

- Take steps to achieve coordinated research efforts across organizational boundaries within NIMH and between NIMH and allied agencies.

- Increase the utilization of intramural research capabilities to provide continuity and conceptualization for the research programs on social problems.

- Take steps to increase utilization of research results.
- Increase the administrative priority placed on seeking resources for research on social problems.
- Increase flexibility in the use of existing research support mechanisms.

Whether we have found a satisfactory middle position between developing research to generate new knowledge and developing research knowledge for utilization in social programs is for you to judge; that is what we attempted.

FOOTNOTES

1/ This presentation was selected primarily from Chapter 11, "Research on Social Problems," in Julius Siegel, *et al.*, Eds., *Research in the Service of Mental Health: Report of the Research Task Force of the National Institute of Mental Health* (DHEW Publication No. (ADM) 75-236, 1975) and from a *Report* from the Social Problems Study Group of the NIMH Research Task Force, July 1973 (xerox), to address the essence of the questions put to me by conference staff. To balance the special perspective of the NIMH scientists and scientist administrators who comprised the Social Problems Study Group, some 20 experts from outside of the Institute and another 20 from within participated in reviewing specialized fields of social problems research, in identifying issues and trends, in reconstructing legislative or organizational history, and in criticizing early versions of our report. Unfortunately, we failed to consult policy-makers, victims, or the citizenry, just as the researchers we criticize have done, and the implications of our omission did not fully strike us until we began synthesizing our materials and formulating recommendations. Since my views occasionally differ from those of other members of the Study Group, responsibility for what follows is mine.

2/ Siegel *et al.*, *op. cit.*, pp. 1-2.

3/ Exclusive of mental illness, alcoholism, drug addiction, and retardation.

4/ Applied Research Branch, Division of Extramural Programs and Division of Special Mental Health Programs.

5/ Division of Mental Health Service Programs and Applied Research Branch, Division of Extramural Research Programs.

6/ Division of Extramural Research Programs other than Applied Research Branch.

7/ In the Division of Special Mental Health Programs.

8/ Glaser, E. M. and Taylor, S. A., "Factors Influencing the Success of Applied Research," American Psychologist 28, 140-146, (1973).

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COMMENTARY

Clark C. Abt
Abt Associates, Inc.

I'm going to try and be deliberately provocative, and if you would like to stand up and work out your aggressions by waving your arms for a few seconds, it might improve the quality of the discussion afterwards.

For it's the failure to really do this very effectively that is our concern today. I think part of it is related to the really tremendous predominance of the disciplinary and university model for the organization of research at NIMH.

Just to make clear where I'm coming from, since it is obviously biased, I come from interdisciplinary, sustained, problem-oriented, output-oriented teams for doing social and economic research. I've tried it both ways, the solitary disciplinary model and the team model. I find the latter a lot more productive.

I think part of the problem at NIMH and some other agencies that are, I think, excessively discipline and university model oriented, is that there are a lot of little research projects undertaken. In about 100 years of education research, we have had thousands of little studies taking one variable or two variables at a time. Why? Because the individual professor with a few graduate students could only encompass a small sample. You can't do much of a multivariate analysis with a small sample so we would look at student/teacher ratio or a particular characteristic of kids or something -- find out something about one variable. No great external validity, we couldn't replicate this in different locations; no great internal validity because we basically simplify the multivariate problem into a single variable problem.

What we have to show for billions of dollars and thousands of small educational research projects is very, very little. We don't know how much kids learn. We don't know what makes really good schools except the judgmental things that we feel intuitively. Educational research has largely been a failure, more so than most research has been a failure. I'm aware that most research has been a failure or it isn't research. There are only a few winners, but there are an unusually small number there.

LARGE VERSUS SMALL SOCIAL RESEARCH

Now, I'd like to suggest to you an alternative strategy which very simply is large versus small social research. I have four assumptions. First, science is the best alternative to executive guesswork in social policy-making. By the way, that is a challengeable assumption. But, I do assume it and I do believe it.

Second, large social programs are needed to deal with large social problems because they are usually multivariate problems affecting large numbers of people in many different locations whether it is juvenile delinquency, mental health, educational development, environmental control, or whatever. These are large problems affecting many people in many diverse ways. You can't really treat them with a single or small number of variables under control.

The third assumption is that social experiments are the most efficient and relatively cheap way of testing whether various treatment modalities work or not. But, more important than evaluation, they are probably our best way of understanding causation for prevention as well as treatment, and for forecasting.

I think the best, most successful area of science here that is interdisciplinary, that involves teams -- sustained, longitudinal research of large numbers, over long periods of time on a big research model -- is medicine. And medicine really does work. Medicine -- particularly the public health part of medicine, the development of various antibiotics and so on, the control of contagious diseases -- is probably the largest success story in the last 100 years in the application of science to social problems. I don't think anything has worked as well as that.

Our education system is basically the same as it was 100 years ago. But medicine is very different. There are some diseases, as you know, like smallpox, that have just been eliminated. The reason social experiments are probably the most efficient way of conducting large-scale research on social problems is that the alternatives are really poor.

The major alternatives are, first of all, model building, theorizing, and so on. We don't have the mathematics now to deal with models that have large numbers of variables in a highly interactive mode. These models blow up. It isn't a matter of computer capacity, it is a matter that we can't really deal with -- the multiplication of errors in such a model.

The other alternatives are small experiments and I've already gone into the reasons why they are not effective.

The third alternative is national implementation of some categorical social action program. That is extremely risky and expensive because if it doesn't work on the basis of theoretical predictions you have wasted enormous resources with great opportunity costs to other possibilities.

Now, my fourth assumption is that social experiments have to be large. Why? First of all, we need large numbers of participants to create the minimum statistical quantities for the cells or the matrix of many variables since this is obviously a multivariate kind of problem.

Second, because we have complex contexts and constraints, very nonhomogeneous populations and locations -- for external validity and replicability we need many different locations. We need significant samples in a large number of locations.

We already have a large number of participants, generally something on the order of 10,000 participants and up. If you multiply that through by perhaps fifty dollars a participant, just for surveying, analysis, and overhead costs, you can see that we are running into millions of dollars and probably years for each particular social experiment.

Third, the reason why they get large is they are intrinsically interdisciplinary because the problems are intrinsically interdisciplinary, and that means you have to have professional and technical skills in depth in five or ten disciplines. So you have research staffs on the order of 50 to 100 people and they have to work several years. We are now talking about large experiments in the 3 to 30 million dollar range.

There are currently about ten of these underway in the U.S. -- in health insurance, income maintenance, housing allowances, education vouchers, experimental schools, early childhood education, day care, defendant diversion, energy conservation and probably a few others that I don't even know about. They generally spend 3 to 30 million dollars a year and they are usually staffed by somewhere from 50 to 500 professionals. We spend collectively somewhere on the order of 100 million dollars a year on these right now.

It is my assertion that we ought to do about ten times as much of this as we are doing and spend about a billion a year instead of 1/10 of a billion on this to really make progress. That is roughly twice the annual social research budget of the entire United States of about $480 million. That is asking a lot. However, I think, productivity would greatly increase.

TEN CRITICISMS OF LARGE SOCIAL EXPERIMENTS

One can think of at least ten caveats to that kind of proposal for building on NIMH's discovery that experimentation should be practiced in social research. There is generally a critique from the policy crowd and the decision-makers that these large social experiments deliver results too late to help to make policy. That is true when they start up on a one-shot basis to answer a question this year when it takes five years to complete the experiment. But if multiple projects were being done continuously there would be a constant stream of research results that could be accessed.

A second criticism is that they are too expensive, but the alternative costs are really much greater. All you have to do is work through the shadow pricing of the social costs of our various social pathologies and they eat up most of the energies and resources of our societies and most of the growth capital, human capital,and physical capital that we generate. The alternatives of ineffectiveness in research or ineffectiveness in problem-solving are much more expensive.

Third, that it is wasteful. Indeed, it is wasteful. The alternatives seem more wasteful.

Fourth, it is disruptive, too disruptive. President Ford particularly feels that people are tired of being surveyed. The empirical data does not really support this. In our surveys of about 15,000 people in housing allowance programs, a major social experiment, less than one percent object to detailed surveying. As you know from detailed research, if you got a response rate of less than one percent it is unlikely to bias your results very much. You can do pretty well with a 20- or 30-percent nonresponse rate. So this is not really a problem. We can just let people who don't want to be surveyed, who don't want to participate in experiments, not participate; and at these numbers we are still all right.

Fifth, big social research interferes with the natural market mechanism for curing our social ills. Yes, but does the natural mechanism really work? Are we letting it work even now? Aren't we interfering massively with a patchwork quilt of little incremental programs, most of which are initiated on the basis of somebody's theory rather than empirical research results?

Sixth, as these large experiments are conducted they are often not really scientifically conclusive and are likely to be misleading: Hawthorne effects, selection effects, and so on. There are very few large social experiments or any kind of social researches that have ideal internal and external validity. We can do a lot better that most of us have done up to now if we just exploit the available state of the art in experimental design. Probably the best kind of review of a lot of the possibilities here in plain language is Campbell and Stanley's Experimental and Quasi-experimental Design, which gives a wealth of approaches to social experiments.

Another argument is that this will annoy legislators who feel that we are not using the available data and resources that have already been granted us. This is particularly common among the more populist sections of our legislatures. Here we have a failure of communication. Most legislators are lawyers and their model of research is legal research -- have a couple of bright guys hit the library and look up precedents. Social research is different. That isn't well understood. They can't understand why it should take more than a year. That is a matter of education which we are responsible for.

Eighth, we are doomed to invalidity by the Hawthorne effect anyway. Well, there are placebo controls that can be designed; and, furthermore, the Hawthorne effect can be used as a policy supportive mechanism.

Ninth, even if we successfully do these experiments it doesn't really change the conventional wisdom. The New Jersey income maintenance experiments more or less found that there are no significant work effects from income maintenance. Most people still believe there is, even though they are aware of the experimental results or they are not aware of them. Again, this is a failure of dissemination, communication, and education with the research results.

And, finally, tenth, the results are not really used by the decision-makers in making social policy. Here again is a failure of education and communication on our part. Some of us, in an attempt to deal with this, have organized a Council for Applied Social Research to try to increase the communication between the researchers and the users of research; between the producers and consumers of policy-relevant, scientific research about social problems. If you are interested in that organization and working with it I would be delighted to talk to you about it. Thank you.

> "I met with the Mayor of Knoxville, Tennessee, and we were talking about a planning study in transportation. He said, 'I don't want any more planning studies for anything! Every study that I have on my book shelf begins with: Knoxville, Tennessee lies between the Great Smokey Mountain Range and the Cumberland Plateau. The average mean temperature is 68° and the annual average rainfall is 70 inches.' He said, 'Hell, I know that!' ... and you know, he (is) right."
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> *Kenneth Heathington*

URBAN TRANSPORTATION: THE REAL ISSUES NEED TO BE ADDRESSED

Kenneth W. Heathington, Ph.D., P.E.
Director, Transportation Center,
The University of Tennessee

I began working in research in 1960, as a graduate student. For about ten years I was very prone to do research for research's sake. It was very interesting -- all the mathematical formulations and models were very intriguing. I communicated with other people in my field and they communicated with me. Nobody else knew what was going on. Then about 8 years ago I began to feel that my contribution to the transportation field must be nil -- that there was not anything happening in terms of making improvements that I had assisted with. I changed my direction completely in terms of what I was going to do and how I was going to do it.

So, for these past few years I have been very active in implementation and effecting change within the transportation field. I have held a political appointment. I have worked very closely with local governments, with mayors, and with governors' staffs and have been offered state level cabinet posts. I have been involved in many things to implement new and better programs in transportation.

From some of the things I hear today I have the feeling that many of us wish the implementation process were better, but many of us are not willing to get into the process to make it change. A university colleague of mine, who is also a transportation engineer, ran for public office about a year ago. This office would have a direct bearing upon making transportation improvements. Out of about 50,000 votes cast he lost by something like 800 votes. It was very close. This individual took a leave of absence from the university of a tenured position. To me this is a dedicated individual who says, "I have to see some changes made and I must become involved." So, some of the things I want to talk about today are examples to illustrate how we can help solve our problems rather than philosophizing on what someone else should be doing.

I do not think we really address the true issues in the transportation field. I do not think we have ever addressed them within the universities. I am not sure we have addressed them very much anywhere. We are seeing a little bit of improvement made with some of our research, but not to a great extent. Transportation has been such an important part of the country's growth that the real issues need to be addressed.

There are many issues or problems in the transportation field. We have tried to address many of these issues with technology and that has not really been successful. You hear a lot of people talk about the urban transportation problem. There is no urban transportation problem. There are hundreds of urban transportation problems.

Transportation has played a very important role in the development of the United States. It has provided for the movement of both people and goods since the very earliest settlers. In early times settlements were located near natural harbors, near rivers or other natural means of providing transportation services. The development of the railroads in the 1800's facilitated the establishment of cities in areas other than near waterways.

Transportation services within cities had not developed extensively in the early 1800's as housing and places of employment were often located together. Many individuals lived above their stores or places of employment, and thus there was not a need for a tremendous amount of intraurban travel. With the development of rail transportation, a more flexible arrangement for the lcoation of housing and employment could be attained. Many large cities developed rail systems on radial routes emanating from the central business district. This system of rail transportation permitted individuals to locate some distance from their place of work.

As one reviews the development of many older cities in the United States, one finds the more densely populated areas along old rail-lines that were built in the 1800's. With the development of the automobile after the beginning of the 20th century, the areas between the radial rail-lines were filled in with housing and other developments. The automobile was often utilized as a feeder service to the rail-lines, thus permitting further development in the outlying areas. Therefore, there was an evolutionary development in the older cities of streets and roads, housing, commercial activities, etc., as the city grew through different eras of transportation services.

THE URBAN TRANSPORATION PROBLEM

As cities increased in population, the need for intra-city travel, of course, increased. Many cities became very complex entities providing services and goods to meet very diverse needs of individuals and businesses. Because of the development of these complex urban structures, problems have arisen over the years with such items as: (1) rapid movement of people and goods, (2) peak-hour movement of people and goods, (3) capacity of facilities, (4) location of facilities, (5) choice of modes, (6) conflicts between private and public services, (7) terminal operations, (8) terminal location, (9) safety, (10) resource allocation, (11) energy, (12) system integration, (13) administration, (14) storage, (15) technology, (16) environment, (17) levels of service, (18) urban development, (19) social consideration, (20) political consideration and many, many others.

One can quickly see that there really is not an urban transportation problem -- there are many problems in urban transportation. One can also see that these problems cover a wide variety of disciplines and must be addressed with many different approaches and with many different types of expertise. The interdisciplinary nature of the problem requires that a system approach be taken when seeking solutions for increasing the mobility of urban residents and the movement of goods.

THE CHANGING NATURE OF CONSUMER EXPECTATIONS IN URBAN TRANSPORTATION

Transportation services have been improved over the years. They have improved for a wide variety of reasons. A hundred years ago, an individual would have been very pleased to be able to mail a letter in Washington, D.C. and have it delivered one or two days later on the West Coast. One would also have been very pleased to have traveled from Washington, D.C. to San Francisco in a matter of six hours or less. Likewise, one would have been glad to be able to drive in an automobile from Washington, D.C. to San Francisco continuously on a divided highway, without ever passing through any stop signs or traffic and roads. One might well argue that only after there is an instantaneous transfer of matter will it be possible to satisfy the consumer's expectations completely.

Transportation is a derived demand. There is no demand for transportation itself. You derive that demand because you want to do something else: you go shopping; you go to work; you take a trip; you take a vacation; you go to a doctor; or, whatever it may be. It is a means of accomplishing something else. It is a joint commodity, so to speak. And because it is a derived demand there is not that much in products -- in terms of purchasing it.

It has to be service to provide you with the ability to do other things. And so the expectations continually rise and increase on it. That is always changing. It is placed upon those of us who work in the transportation field to continually upgrade the system to meet the consumer expectations. It is an iterative process. Each time we upgrade it the expectations get higher. As the expectations get higher we try to upgrade it. We upgrade it and the expectations get higher.

One must realistically view the fact that as travel times are reduced substantially between any two points, the marginal return on the investment for further reduction of travel times becomes minimal. Thus, the expectations for improvements in urban transportation should not lie primarily with the reduction of travel times and congestion, but in other areas such as the reduction of air pollution, noise, accidents, death rates, etc. A realignment of priorities for improvements would redirect the allocation of resources for making improvements in urban transportation.

THE COMPETITIVE NATURE OF URBAN TRANSPORTATION SERVICES

There is both competition and restriction of competition between the public and private sector in the urban transportation field. The automobile competes with public transportation services and private enterprise such as taxis, limousines, etc. compete with publicly owned systems. Highways are normally publicly owned, but used by the private sector. Many private firms compete with one another for carrying goods and people. Taxis and trucking firms are often in competition for a particular market. In some instances, tax money is used to provide competition from the public sector with private carriers. The services provided by public carriers are often at less than the true cost. This, of course, puts the private carriers at a disadvantage.

Let me give you some illustrations of this competition. In one city with which I am familiar the city desired to develop a program for senior citizens. This is a worthwhile endeavor. In this program they hired their own drivers and bought their own vehicles with federal money. They are up to about 600 people per day in ridership, supposedly, and the program has substantial costs. The service is free to those eligible to participate.

One of the local taxi companies has now threatened to sue the city because they feel that their business has decreased because of the program. Some of the other taxi companies are also trying to enter in the suit. The taxi companies have attempted to work out an arrangement with the city where the program would use taxi services rather than purchase vehicles and employ drivers.

This example, of course, is a direct conflict of private and public agencies with public tax monies being used to decrease the amount of revenue of the private enterprise sector.

Let's take another example. I worked with a small city in Tennessee for several months. I met with both councilmen and the city manager's personnel responsible for a senior citizens program. They, too, wanted to purchase their own vehicles and hire their own drivers and dispatchers. This would have caused a conflict between the local cab companies and the city. A program was finally developed that would incorporate the local taxi companies into the program. Senior citizens buy transportation tickets for 25¢ on the dollar. The cab companies bid the service at 90¢ on the dollar. In other words, each time a dollar coupon comes in, the cab companies only receive 90¢. The city pays the remaining 65¢. They let both cab companies participate in the program.

Last year there were about $24,000 worth of services generated for senior citizens. Only about $15,000 (65% of the cost) had to be borne by the city. 25% of the cost was borne by the senior citizens themselves and 10% was borne by private enterprise.

Private enterprise will make their contribution, but, you have to know how to work with private enterprise to know how to get them to make their contribution. This program avoided the conflicts that occurred in the other city. Everyone (city, taxi,and senior citizens) has expressed pleasure with the program.

In some instances, competition is prohibited between private and public carriers. That is, a publicly owned carrier may have an exclusive franchise which prohibits the private carriers from being in competition. Competition in the true nature of the free enterprise system is good and in most instances will lead to the provision of better services at lower costs to the consumer. However, in the transportation field there is not a completely free enterprise competitive system in operation. Often within a given mode, there is an exclusive franchise awarded which prohibits or reduces the competition within that mode. While there is competition existing between modes such as the automobile and public transportation, one finds that competition within a mode may be nonexistent.

THE REGULATORY NATURE OF URBAN TRANSPORTATION SERVICES

Transportation is one of the more regulated industries in the United States. Such agencies as the ICC, FAA, CAB, Public Service Commission, regional agencies, and local city councils are a few of the well-known regulatory agencies. These agencies regulate both the movement of people and goods. They regulate air, rail, public transportation, taxis, and all forms of services provided in the transportation field. Because of the vast amount of regulations which exist in the transportation field, innovation is difficult if not impossible to achieve. Generally, the amount of innovation that can be provided in any industry will be inversely proportional to the amount of regulatory control placed on that industry. In the transportation field, there are many firms that carry goods, but cannot transport people. Companies or firms carrying people cannot necessarily transport goods.

Innovation should come, from all places, the university. However, we have many faculty members who stand behind our ivy covered walls and will not go into the field and do anything because if you do anything somebody will find out about it. If it happens to be bad you'll be criticized. But, it is easy to stay on campus and write reports. It is so easy to sit back in your office and generate thousands and thousands and thousands of esoteric pages -- just as easy as to philosophize.

When you sit down with the city council or with the mayor to give solutions to problems, you must be reasonably pragmatic. I met with the mayor of Knoxville, Tennessee and we were talking about a planning study in transportation. He said, "I don't want any more planning studies for anything! Every study that I have on my book shelf begins with: Knoxville, Tennessee lies between the Great Smokey Mountain range and the Cumberland Plateau. The average mean temperature is 68° and the annual, average rainfall is 70 inches." He said, "Hell, I know that!"

And you know, he was right. He was just as right as he could be. It didn't make any difference whether that report came from a university or from a consulting firm. Everybody here, including myself, is just as guilty of that as he can be. He also said, "I don't want any more of that stuff. Tell me what to do to solve my problems." We must become involved in a meaningful way -- in a manner that can be accepted by the layman.

In most states it is illegal to have car pools if the total money exchanged between the driver and passengers is greater than the cost of the operation of that vehicle. A woman in California had to take her case all the way to the State Supreme Court. We are finally getting a change in a few states.

It is illegal in many states to have van pools. We had to work with the state legislature in the state of Tennessee to get the law changed. We must be willing to work with all groups at any level of government to insure that improvements are made in the transportation field.

The cost of initiating change through regulatory agencies for a small company can be in the tens of thousands of dollars.

Thus, the smaller carrier cannot normally compete and thus competition is reduced. Overregulation tends to protect the very large carriers and reduce the number of smaller firms desiring to enter the transportation field. It is interesting to note that of all the modes of transportation, the highway field is probably the least regulated. It is also the one which has tended to expand the most and provides by far the majority of travel for the population of the United States.

Let me offer one other example of regulatory problems. In Knoxville, the publicly owned system has a bus service and a franchise to operate in the area, which means if you have a franchise, no one else can operate. There was a local area which is a public housing area -- low-income, black area -- which needed public transportation services. Some citizens came and requested from the KTA Board (Knoxville Transit Authority) to provide services. KTA initiated a new service to the area and they discovered that there were not a lot of patrons. So, they discontinued the service.

A private bus operator said, "I will provide that service at the fares which you charge with KTA. I would like to have the business."

KTA replied, "Why, you can't do that. We have an exclusive franchise." So what happened? No service was provided. The private operator was not permitted to. The public didn't get any services, but in the name of regulation we solved the problem. The franchise wasn't violated.

THE CONSUMER ORIENTATION OF URBAN TRANSPORTATION

We have not really changed in our consumer orientation toward transportation in many, many years. Public transportation, as an example, really hasn't changed in a hundred years. About the only difference between public transportation now and a hundred years ago is that a hundred years ago it was powered by a jackass and now it is powered by a diesel engine. That is about the only difference. One has the same routes that the trolley lines had.

Most consumers in the United States have a choice for urban transportation services. For example, one may choose the private automobile, various forms of public transportation, or shared travel with others such as in a carpool, vanpool, etc. One often hears of the captive ridership which is forced to utilize some form of public transportation. However, the trip making of this captive ridership only represents from one to two percent of the total trips being made in an urban area.

It is only within the automobile mode that a consumer preference has been utilized in developing transportation services. With other modes, such as mass transportation, a product is developed and then an attempt is made to market the product to the consumer. This type of market development is similar to that utilized by a product-oriented firm. A product-oriented firm is one that develops a product and then attempts to sell that product to consumers. A consumer oriented firm is one which attempts to determine consumer desires or needs and then develops the product to meet these needs or desires. Historically, product-oriented firms have tended to become unsuccessful over time; whereas consumer-oriented firms tend to remain viable over time by doing what is necessary, that is, change, and thus meet the changing desires and needs of the consumer.

As long as there is a choice that can be made by the consumer, all urban transportation services must become consumer oriented if they are to receive a reasonable share of the travel market. The amount of the travel market that a particular mode of transportation receives is an indication of the consumer acceptance of that mode. One sees a very small amount of the public utilizing these services. (About 2-4% of urban trips are made by public transportation.) This is an indication that there is little consumer acceptance of the particular types of services being offered.

THE LACK OF COORDINATION OF TRANSPORTATION SERVICES

Urban transportation systems are generally uncoordinated and fragmented in the United States. Normally there is not an interface between automobiles, taxis, buses, rail, or any other mode or service found in an urban area. An individual cannot travel about an urban area utilizing a variety of modes. An individual is generally confined to one mode or type of service for travel needs on a single trip. There are, of course, minor exceptions to this in certain urban areas but in the majority of cases transportation services are uncoordinated.

If one has a lack of coordination with other types of services in urban areas, one can quickly see the impact that this strategy would have. Suppose there were five or more phone companies in a given urban community with no coordination or interconnection of these systems. One could only place a call to a certain part of an urban area and could not communicate with other portions of the urban area because of the lack of coordination. Further suppose that water systems, mail services,or a host of other urban services were not coordinated or interconnected. It would seem to be unimaginable that these types of urban services should not be coordinated to provide a high level of service for all urban residents; yet this same concept does not apply to the transportation field either in the movement of goods or in the movement of people. If one addressed the problems in urban transportation, a lack of coordination is one of the key problems to be addressed.

REGIONAL DIFFERENCES IN URBAN TRANSPORTATION NEEDS

There are significant differences among regional needs within the United States relative to the needs of providing urban transportation services and to the manner in which these services are provided. Cities of the North and Northeast are generally older, more densely populated, have heavier corridor movements, different street patterns, much colder weather in the winter, and the people have a more positive attitude toward the utilization of public transportation. Cities of the South and Southwest generally are newer, that is, they were formed after the advent of the automobile, are less densely populated, have somewhat different street patterns, much warmer weather in the winter, and the people have a less positive attitude toward public transportation.

The cities of the South and Southwest are heavily automobile oriented. They were developed after the advent of the automobile and cities such as Houston, Dallas, and Phoenix have developed street patterns which are conducive to the utilization of automobiles for all types of travel. Cities of the North and Northeast such as Chicago, New York, and Boston are all older cities that have oriented themselves more toward the utilization of public transportation systems to provide many of the transportation services.

The solution to transportation problems in one city may not necessarily be a solution for another city. Thus, it is difficult to develop a strategy for problem-solving that will have application for all urban areas within the United States.

THE IMPACT OF TECHNOLOGY ON URBAN TRANSPORTATION

Technology has not really had a significant impact on urban transportation. In reality, there is little change in the technology being utilized by various modes of travel during the past 50 years. Rail systems today have about the same technology as was in existence 50 years ago. (There are only a few exceptions.) Buses also have similar technology. There is not really much difference in the technology of automobiles found on city streets today than those found in the 1940's or shortly after World War II.

Let me give you an example. In Morgantown, West Virginia it was politically decided to put in a personal rapid transit system. At last official count (and the official count is estimated to be far less than the real count), there was something like 65 or 70 million dollars spent on the PRT system. Some estimates indicate that the cost will go to 100 or 150 million dollars to build 2.4 miles of the PRT system.

What the system does is connect two of the university campuses in Morgantown. (You might move the whole university for 125 million dollars.) They are going to charge the students a fee to pay the operating costs. It is questionable if the demand will pay the operating costs, much less pay for the capital expenditures.

We have had aerospace engineers who said, "We got to the moon and we can solve the urban transportation problem." If we could have the same budget per passenger mile as for the space program, anyone could solve the urban transportation problem. Technology will not in and of itself solve our transportation problems.

With the decrease of the space programs, many of the space industry personnel felt that the urban transportation problems could be solved with space age technology. Many proposals for PRT systems, automated guideways for automobiles as well as other new and innovative technology-oriented systems are made. Some of these proposals were funded and systems technology was developed and implemented. A product was developed without regard to consumer preferences and then the product was to be marketed. There was very little analysis performed to determine the consumers' needs, desires, and expectations relative to a particular type of product (i.e. transportation service). Since many of these new systems that are oriented toward technological innovation do not address the many problems in urban transportation, it is questionable that they will have a significant impact upon improving mobility in an urban area.

The solution to the many problems in urban transportation does not lie with science and technology. The problems that have been previously shown to exist must be addressed by other areas than technology. Unless all these problems can be addressed together, very little can be done to improve transportation services in an urban area. Science and technology play only a supporting role and should not be considered as the dominant force in the solution of these problems.

RESEARCH AND DEVELOPMENT ON URBAN TRANSPORTATION PROBLEMS

It is questionable how successful research and development has been in solving urban transportation problems. There are many millions of dollars expended each year for transportation research and development by the public and private sectors. It is questionable how much of an impact these research dollars have really had on solving urban transportation problems. Much of the public resources are utilized for improvements in technology. As an example, in the highway field many millions of dollars are spent to improve concrete and asphalt pavements, signing, materials, and other technological needs. In the transit field, much of the research monies are spent to develop better buses, rail, etc. Relatively speaking there is little spent on the real problems in urban transportation. There are some monies allocated to addressing other issues than technology, but on a relative scale, these appear to be minor. The Arabs did more to increase ridership on public transportation systems than all of the hundreds of millions of dollars that we have spent on research. And they did it in about three months.

Transportation services must be efficient, effective, and economical. A service that is effective may not be efficient and/or economical. A service that is economical may not be effective and/or efficient. Most of the research that is emanating from the public sector generally addresses only one of these characteristics. If one is to be successful in the transportation field with research monies, all three of the characteristics must be addressed for any given system. There must be a change in the research emphasis if improvements are to be made in urban transportation.

THE NEED FOR AN INTERDISCIPLINARY APPROACH

The problems in urban transportation span such fields as engineering, political science, sociology, law, economics, health, education, business, energy, environment, and many others. In fact, it is very difficult to find a field that in some indirect manner does not have a bearing upon problems in urban transportation. There is no single field that will be able to solve the problems in urban transportation. There must be an interdisciplinary approach to solving the problems if permanent and meaningful solutions are to be found. There has generally been a lack of interdisciplinary approaches taken to solving urban transportation problems. This applies not only to the implementation of new services and systems within an urban area, but also applies to research and development.

There has not been effective management of interdisciplinary research by universities or by private organizations. While at times interdisciplinary approaches have been attempted, the management of these projects has often been less than desirable. This is partially due to the fact that universities are not really training students in large numbers to function in an interdisciplinary environment. Universities are still very oriented along disciplinary lines and do not encourage interdisciplinary activities within their framework. There must be a change at the basic level of learning in order for interdisciplinary approaches to have an impact in the urban transportation field or, for that matter, in any other field. In reality, there is much lip service paid to interdisciplinary approaches to problem-solving, but little meaningful efforts are actually directed toward interdisciplinary approaches.

SUMMARY

Science and technology cannot in and of itself solve the problems in urban transportation. The problems are too numerous and diverse and span too many disciplines. There must be an interaction of many disciplines to make a meaningful contribution to problem-solving in urban transportation. One should begin by broadening the educational backgrounds of university graduates by training them to be productive in an interdisciplinary setting. This, of course, requires a structural and philosophical change in educational programs in institutions of higher learning. This will not occur to a great extent in the near future. There are, of course, universities that have oriented some of their programs toward developing students to function in an interdisciplinary environment. However, the majority of universities are still oriented along disciplinary lines and are not training their students to function as a member of a team that can address a wide variety of problems.

There are tremendous amounts of resources being ineffectively utilized because of a lack of appreciation and understanding of urban transportation problems. There are many people who do not comprehend the various problems that exist and attempt to allocate resources to the solving of one small problem in urban transportation. There must be a broader approach to problem-solving in the future than has been in the past. It requires a diversity of background and educational experience in order to address the many issues that exist in an urban area relative to transportation. However, without the addressing of the many issues by wide variety of backgrounds and expertise, one can never hope to make a meaningful impact upon solving the needs for travel of urban residents.

NOTES ON APPLYING SCIENCE TO PUBLIC PROBLEMS:

THE EMERGING STRUCTURE OF INTERDISCIPLINARY EFFORTS

Christopher Wright
Office of Technology Assessment, U.S. Congress*

INTRODUCTION

Two considerations underlie the present demand for interdisciplinary inquiries. First, there is reluctant acceptance of the fact that knowledge produced within the specific scientific disciplines is seriously limited in its direct social applicability. Second, the wishful thought persists that new amalgams of disciplined knowledge, although generated in the same milieu, may nevertheless contribute decisively to our society's effective and responsible management of its affairs. The hope persists that the quality, durability, and credibility traditionally associated with the disciplines can be retained throughout the process of applying science to public problems. In fact, the applications of scientific knowledge are not likely to remain that simple and direct. The dimensions of society's problems seldom coincide with the boundaries of academic disciplines or any combination of them. There is no way within existing disciplines to ascertain the full public or social significance of a particular bit of scientific knowledge. The changing nature of societal problems also reduces the overall usefulness of the established technical professions.

Nevertheless, there is now an emerging structure to knowledge in action that may help overcome these limitations. It involves systematic studies of a distinctly intermediate nature. Knowledge derived from these studies is concerned with the ongoing interactions between the separate worlds of scientific knowledge and social action. This kind of knowledge tends to be oriented either to real social problems and their solutions or to equally real issues of public policy. That which is problem-oriented is most evident. It includes the know-how associated with the management of ongoing, knowledge-intensive public enterprises, such as those associated with the maintenance of national defense capabilities, with major medical and health care delivery systems and with such focused responses to social interests as the Manhattan Project and the Apollo Program. In such situations, public-oriented knowledge may not differ in kind from that developed as applied science to meet the kinds of problems which private clients have and which the engineering, medical and other professions are prepared to deal with. Only when such problem-solving approaches to social needs misconstrue the problem, or proceed in ways that tend to aggravate conflicts among social values or interests, does it become evident that problem-solving approaches are not able to cope with the consequences of their prescriptions for the overall promotion of human values and social goals, and that there is a pressing need for prior knowledge about second- and third-order systemic effects of such activities.

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* The views expressed in this paper are solely those of the author and in no way represent any position of the OTA.

A generalized capacity for public problem-oriented studies of this nature is not yet well developed. It will not come easily. The current efforts of the National Science Foundation and other institutions attempting to cultivate such a capacity would indicate as much.

The policy-oriented form of intermediate knowledge is still less well developed, although it concerns matters that are just as real and important as the social problems that are potentially solvable. Policy issues may be resolved, but are less often solved. They tend to be changeable and elusive. The difficulties involved in acquiring policy-oriented intermediate knowledge are illustrated by those encountered in formulating informed, coherent, and pointed environmental impact statements or technology assessments for policy-making purposes.

Thus far, efforts to acquire and apply policy-oriented knowledge are very much in the shadow of problem-oriented studies. They could easily remain so, for knowledge about the applications of science to meet defined social objectives is relatively accessible compared to that about alternative ways to attain social objectives, to reconcile them, and to enhance social values, or about the implications of doing so. And yet, as scientific knowledge is applied more systematically to public problems, it will become increasingly important to differentiate these two kinds of studies, both conceptually and institutionally.

Neither form of intermediate knowledge can be derived from the science disciplines alone or in combination. They not only cut across the disciplines but must take account of current and future contingencies, expectations, value preferences, resources, and interests which may not be reliably and precisely predicted or of enduring significance. Knowledge of such uncertainties is not likely to conform to established forms of scholarship, but what could amount to a new, transcendent kind of discipline may arise, based on the techniques used to recognize and incorporate such factors in problem-oriented and policy-oriented studies.

It is particularly important to highlight policy-oriented studies because the underlying concepts and the institutional structures supportive of such studies are beginning to emerge as the central, although least stable, element in the framework available for adapting science to social needs. Their centrality is not due to their present impact or inherent quality, but to the prospect that our society must depend on them as it strives to meet the pervasive and persistent need to assure real variety of public policy choices and informed understanding of them. This requirement of a democratic society is especially difficult to meet in those areas of social concern where advances in science and technology and their ready application tend to foreclose policy choices or else place the interpretation of social needs and the determination of ways to meet them in the hands of unwitting, if not unwilling, specialists and specialized agencies.

A CONCEPTUAL FRAMEWORK

As knowledge of the world and attention to human needs increase, the characteristics of many social activities and functions are becoming increasingly differentiated, refined, and specialized. On the one hand, advanced procedures for scientific inquiry aimed at discovering enduring truths are extending the scope, depth, and coherence of organized knowledge and trained expertise. On the other hand, there has been a complementary concentration and focusing of major societal decision-making, leading to specific capabilities and responsibilities for large-scale initiatives and for formulating and implementing effective policies in areas hitherto governed by tradition or by momentary inclinations. Ever more public interests and actors are being identified or created as potential clients, as well as beneficiaries, for applications of scientific knowledge.

The strongest links between the world of action as a potential client for knowledge and the world of scientific knowledge as an expanding resource put science in the position of handmaiden to the recognized interests and objectives of private and public clients such as those concerned with preservation of health and national sovereignty. This is a problem-solving role. Much less well developed, and the focus here, is the use of scientific knowledge to expand and clarify the range of choices which determine a society's needs and the ways in which science can help to meet them.

The framework suggested in the following schematic diagram (Figure I) is proposed for the purpose of distinguishing three patterns of application and locating seven types of studies within a meaningful conceptual framework. This interpretation of the emerging structure of intellectual studies need not be oversimplified or unnecessarily complicated.

Most of these seven types of studies have already been referred to. Policy-oriented studies, although least developed as a type, are closely linked to, and in a sense surrounded by, the six other types of studies. These are distinguished one from another by their primary commitments and orientation. Discipline-oriented studies contribute to the reservoir of basic scientific knowledge. Decision studies are closely tied to the immediate responsibilities and options of a client. Problem-oriented studies are concerned with real problems which lend themselves to scientific and technological solutions, as distinct from those social problems requiring reconciliation or policy choices.

The kinds of immediate knowledge involved here tend to draw from the formalized problem-solving studies which dominate the training and practice of persons functioning as professional physicians, engineers, agriculturalists, foresters, and so forth. Such studies, in turn, provide a basis for carrying out feasibility studies closely tied to a public client's need to assess the feasibility of its adopting one or another solution. Discipline studies are concerned with refinements in the techniques and theories which tend to guide the future growth of a discipline. They are the principal arena for scholarly recognition. The present close ties between such studies and discipline-oriented studies probably account for the limited interest, thus far, in discipline oriented studies that are explicitly pursued in support of policy-oriented studies.

INSTITUTIONAL ASPECTS

Each type of inquiry requires its own kind of institutional underpinnings. Some, most notably those that are discipline-oriented or problem-solving, are well established in their own professional terms. Feasibility and decision studies are clearly and closely client-related. Problem-solving and problem-oriented studies may also receive the temporary support of strong, especially interested organizations, whereas policy-oriented studies and perhaps discipline studies as well must be based on more disinterested, longer-term considerations and thus depend on quite different kinds

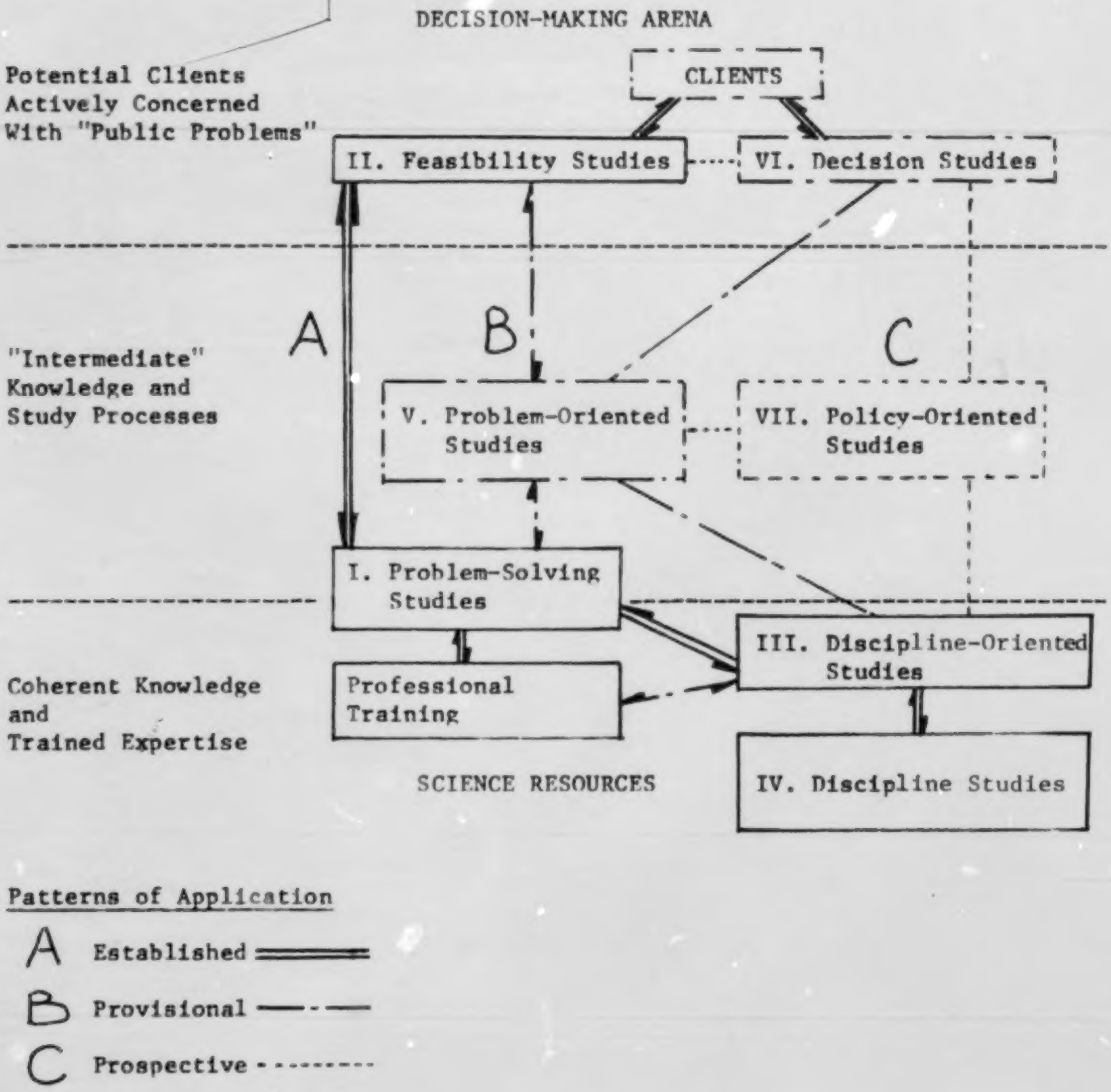
DECISION-MAKING ARENA
Potential Clients Actively Concerned With "Public Problems"
CLIENTS
II. Feasibility Studies
VI. Decision Studies
A
B
C
"Intermediate" Knowledge and Study Processes
V. Problem-Oriented Studies
VII. Policy-Oriented Studies
I. Problem-Solving Studies
III. Discipline-Oriented Studies
Coherent Knowledge and Trained Expertise
Professional Training
SCIENCE RESOURCES
IV. Discipline Studies
Patterns of Application
A Established
B Provisional
C Prospective

FIGURE I: SCHEMATIC DIAGRAM OF INSTITUTIONAL NODES GUIDING THE APPLICATIONS OF SCIENCE TO PUBLIC PROBLEMS

of supportive arrangements.

Particular applications of knowledge to societal problems are also affected by the overall pattern and balance among these seven types of inquiry. Together they determine the vitality of science resources as bodies of coherent knowledge and trained expertise capable of being linked by means of intermediate knowledge with the decision-making arena involving potential clients actively concerned with public problems.

The emerging structure of these efforts is a consequence of an ongoing evolutionary process of differentiation among institutional arrangements and of intellectual coordination aided by an overall understanding and appreciation of this structure and its ramifications.

1. Problem-solving studies are usually conducted by technical specialists established on a professional basis or by scientists acting in that capacity for a specific client. The techniques used tend to follow rather than anticipate new problems. They are those acquired in the course of a training which emphasizes technical solutions to recurrent problems. As prospective clients' perspectives and expectations expand, the professions may then draw more on the sciences, making use of them on a multidisciplinary or even an interdisciplinary basis in order to advance solutions to the problems as more broadly perceived by the clients. Thus the traditional clients of engineers now expect analyses and plans which take account of economic, regulatory, and environmental considerations.

One way or another, much of the advanced work in educational and training institutions is directed to the preparation of individuals for careers involving more or less routine problem-solving for either the private or the public sector. Moreover, in agriculture and then most notably in medicine, universities have assumed continuing responsibility for problem-solving studies and applications through their affiliations with agricultural research, extension services, and medical centers. At times they have also established and taken responsibilities for applied science laboratories designed to solve public problems related to national defense and are under pressure to do likewise with respect to community and urban services, and even global development problems.

2. Feasibility studies are typically conducted for a decision-making body by the equivalent of a budget, planning, or comptroller's office. They may involve sophisticated assessments of a proposed plan of action in terms of efficiency, resource allocations, cost-effectiveness, or the secondary

consequences of the plan. Such studies also include those initial inquiries which, whether done well or poorly, effectively translate a perceived need into a particular set of technical problems expected to have an acceptable solution. As a practical matter, the need to meet an "energy crisis," for instance, may be perceived as a problem of supply or one of demand. Depending on whichever approach is adopted, quite different technical services may be drawn upon with correspondingly different solutions advanced.

3. *Discipline-oriented* studies center on those intellectually challenging problems selected for study as the major activity of a scientific discipline. The expectation is that the resulting discoveries will advance general understanding and are opportune for the application of the discipline's primary techniques of investigation. Because academic institutions are structured to facilitate this pursuit, the contributions of scientists to policy-oriented studies or to decision studies tend to be sporadic and selective. "Targets of opportunity" are seized when they are only likely to result in credible and useful, albeit sometimes unorthodox, applications of science to public problems.

4. *Discipline* studies aim to refine the analytic techniques and fundamental theoretical structures that are the hallmark of a particular scientific discipline. Because the reward systems within each discipline encourage individuals and institutions to advance such studies directly or through discipline-oriented studies, the disciplines tend to be correspondingly less attentive to their potential contributions to the other kinds of studies included in this schema. Discipline studies involving philosophers and historians of science in independent analysis, interpretation and criticism of the techniques and foundation assumptions of a science discipline may even have to be kept apart from the relevant discipline-oriented studies.

5. *Problem-oriented* studies have emerged in response to the need to anticipate and address complex socio-technical problems that are recognized as inappropriately solved, if not insoluble, within the framework of the particular techniques and doctrines of any one profession or discipline. The aim is to acquire knowledge that is likely to be relevant and important even though it would be premature to advance specific solutions.

Nevertheless, the aim in both problem-solving and problem-oriented studies is to supply optimum solutions to complex problems rather than to expand options and the expressions of value considerations associated with policy-oriented studies. Hence, the call for multi-disciplinary or multi-professional inquiries and the suggestion that while the problems fall between disciplines, they may be amenable to interdisciplinary inquiry. Thus far the belief persists that such inquiries will succeed if representatives of relevant intellectual traditions can be brought together.

While such representation may be a necessary condition, to advance solutions in complex situations, it is certainly not a sufficient one. A synthesis of knowledge from different disciplines requires study environments that have proved extraordinarily difficult to secure.

Recognition of the need for problem-oriented studies has outpaced our capacity to meet the need. The Congress has recognized this need in the context of concerns about the environmental and other impacts of technological programs. Present impact assessment efforts tend to be an adjunct of predominantly problem-oriented approaches, although their potential contributions to policy-oriented studies are at least equally significant. A limiting factor is our institutional capacity for conducting problem-oriented studies. Institutions of higher education and research have proved remarkably unadaptable, although not unresponsive. The RAND Corporation is often cited as a major and most promising institutional model. Comparable institutional developments related to non-military public problems display much more variety, ranging from the National Institutes of Health, the late New York City - Rand Institute, and independent contractors or university affiliated centers working on problems of urbanization, transportation, energy, education, etc., to the operating programs of private foundations such as those of the Rockefeller Foundation having to do with the conquest of hunger and population stabilization.

Typically, efforts to acquire "intermediate" knowledge crucial to the solution of a major public problem require substantial resources. And yet, the unpredictability of the kinds of solutions likely to be reinforced by problem-oriented studies results in equally insecure commitments to such a quest. The contributions of no specific profession or science discipline can be assured in advance. The many attentive action-oriented potential clients or beneficiaries are equally unsure of their interest in whatever solutions may be forthcoming. Institutional arrangements for problem-oriented studies are not securely established and may be inherently unstable or vulnerable to exploitation.

6. Decision studies* aim to identify and analyze policy options available to a particular decision-maker under specific circumstances. This is a task for legislative and executive policy planning staffs. They must weigh the costs and benefits of the options in terms of the responsibilities, needs, and resources of the respective policymakers. The dominant considerations are more likely to be legal, political, and economic than scientific or technical, even though the particular public problem and the available options may be closely tied to various highly technical factors or scientific considerations.

The nature of such studies usually requires that they be done by a few persons in a very short time. Thus they must be conducted on the basis of knowledge available from clearly pertinent policy-oriented studies or previously incorporated into the understanding and perceptions of the participants and those who will use the decision studies, perhaps by means of retrospective assessments or training exercises.

The quality of decision studies thus depends to a large extent on the prior education of those who conduct them and on the general state of policy-oriented studies in the particular area. It is also enhanced by the development and use of new forms of policy deliberations and consulting relationships involving systematic awareness of decision processes and the importance of relating them to present choices and long-term goals.

7. Policy-oriented studies are the least developed type of study contributing to the application of science to public problems. They are easier to describe prospectively than to identify in practice. The intent, as in the work of such ad hoc efforts as the Ford Foundation Energy Project, and the Commission on Critical Choices, is to advance knowledge and understanding of the facts and possibilities closely associated with such decisions.

Although the work of the academic science disciplines tends to be oriented more towards problem-solving and the advance of the disciplines than towards policy issues, demonstrations of the social changes resulting from scientific knowledge and the new technologies and related impacts on values and policy preferences have stimulated the demand for policy-oriented studies. The related extra curricular initiatives of scientists have done much to demonstrate the feasibility of such studies.

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* While this kind of study is often described as a "policy study" that term is not used here in order to avoid confusion with other types of studies also contributing to the application of science to public policy and public problem-solving and here identified as policy-oriented.

Thus far such studies have usually been conducted on an ad hoc basis or as an adjunct to problem-oriented studies. As yet there are relatively few institutional mechanisms specifically designed to encourage and facilitate policy-oriented studies on a systematic, impartial, scholarly, and timely basis.

Nevertheless, the perceived need for such capabilities has no doubt contributed to the creation or growth of such unusual institutions as the Brookings Institution, Resources for the Future, and the Institute of Medicine. It has also given rise to other recurring proposals for independent and credible centers for studies of national policy alternatives, including the suggested Institute for Congress.

These proposals differ in many respects having to do with such matters as openness, authority, and accountability. However, they also reflect a constructive common concern that there be more secure sources of credible, independent, analytically based judgment about the dimensions of forthcoming policy issues and related choices and about the knowledge and procedures that will be most useful in resolving them.

SOME IMPLICATIONS

At present the distinctive types of studies and connecting links here identified are easier to distinguish in principle than in practice. Although the same centers of inquiry often perform more than one function, the separation of institutional commitments is important. Figure II, with its illustrative assignment of studies related to national energy problems and policies illustrates the likely ambiguities in the categorization of any particular inquiry and the confusion of functions. These are to be expected when the task is to anticipate and accelerate emerging patterns of intellectual activity and institutional evolution.

The central theme of this paper is that policy-oriented studies are emerging as a distinct form of independent intermediate study, thus making possible the third of the three patterns of application labeled A, B, and C in Figure I. This schema also suggests that certain distinctions within and between science and scientists are likely to become

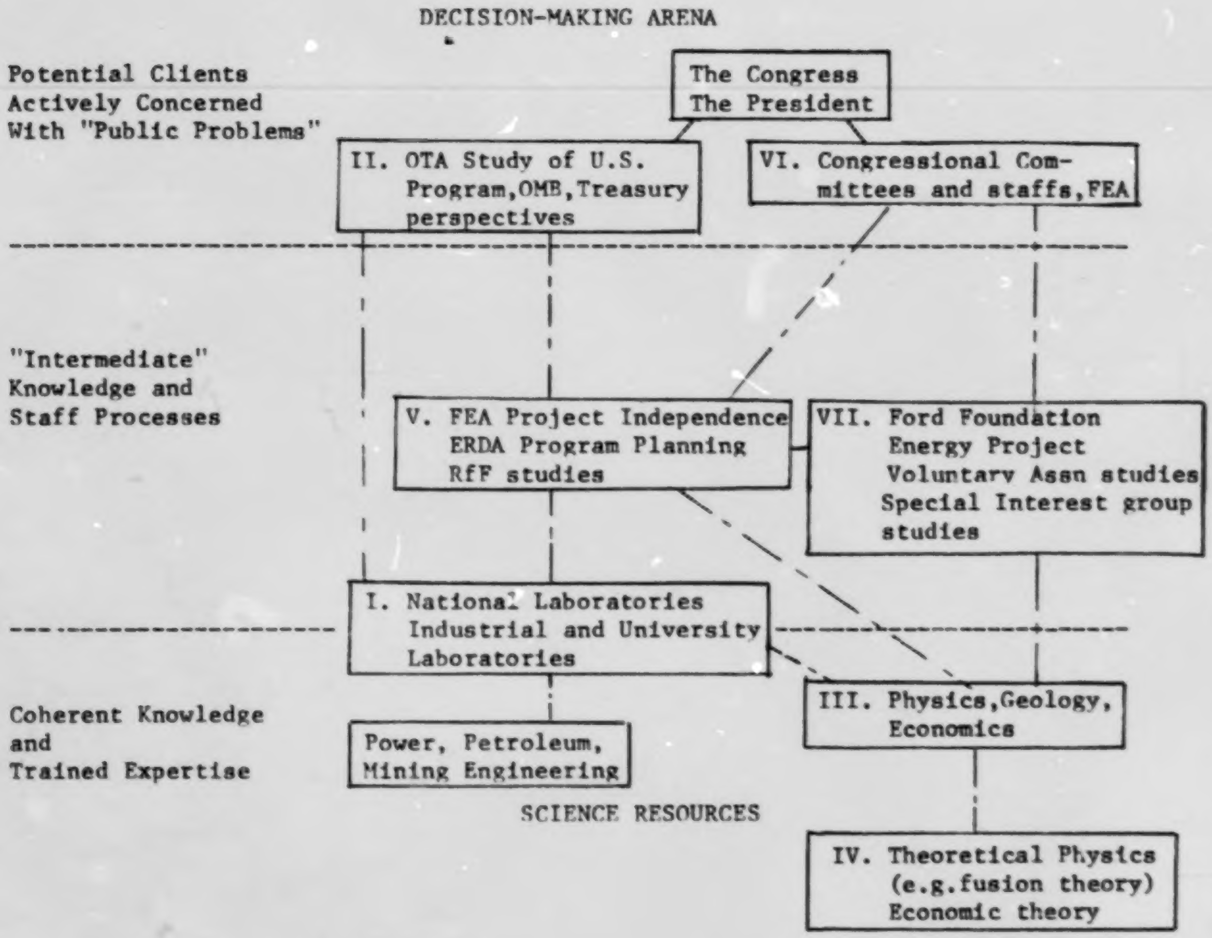
DECISION-MAKING ARENA
Potential Clients Actively Concerned With "Public Problems"
The Congress
The President
II. OTA Study of U.S. Program, OMB, Treasury perspectives
VI. Congressional Committees and staffs, FEA
"Intermediate" Knowledge and Staff Processes
V. FEA Project Independence
ERDA Program Planning
RfF studies
VII. Ford Foundation Energy Project
Voluntary Assn studies
Special Interest group studies
I. National Laboratories
Industrial and University Laboratories
Coherent Knowledge and Trained Expertise
III. Physics, Geology, Economics
Power, Petroleum, Mining Engineering
SCIENCE RESOURCES
IV. Theoretical Physics
(e.g. fusion theory)
Economic theory

FIGURE II: ILLUSTRATIONS OF STUDY FUNCTIONS IN TERMS OF NATIONAL ENERGY PROBLEMS AND POLICIES

more pronounced as these three patterns are differentiated and developed. Some of the ramifications are indicated in the following observations.

Identifying Policy-Relevant Knowledge.

Policy-oriented studies, in contrast to problem-oriented studies, accentuate inherent differences in the import, for purposes of policy analysis, of any specific scientific knowledge. In a problem-oriented study the aim is to devise solutions drawing on the totality of knowledge and "know-how." The knowledge brought to bear must be viewed comprehensively. The aim in policy-oriented studies, on the other hand, must be to isolate those few scientific findings or technical facts, if any, that will make a critical difference in shaping specific policy issues and options. This use of science requires added skills running counter to scientists' sense that because any bit of knowledge might be germane, policy-makers and their staffs should comprehend as much science and technology as possible. It calls for increased sensitivity to the concept of policy-relevant knowledge and to the development of techniques for identifying such knowledge.

Priorities within the Science Enterprise.

The unfamiliar concept of policy-relevant knowledge is thus central to policy-oriented studies. So is the type of imaginative, speculative inquiry that makes use of science to increase potential policy options. Both add new bases for identifying priority areas of interest and inquiry within the body of organized scientific knowledge. Approaches to science incorporating these considerations thus appeal to the side of scientific inquiry bordering on philosophy, the humanities, and science fiction more than do the approaches which emphasize targets of opportunity for expanding and testing, within a discipline, the internal coherence of a given body of scientific knowledge.

Independent Intellectual Institutions.

In terms of supportive organizational arrangements, policy-oriented studies are likely to be incompatible with problem-oriented studies. Policy-oriented studies suffer the most from lack of a well-established independent mission. Efforts to enhance the range of policy choices add unwelcome complexity to the efforts to understand the problems involved in coping with complex interdependencies. Moreover, expansion of policy options depends to a great extent on stimulating informed criticism of the assumptions underlying conventional technical solutions in a given area of social problems.

Traditionally, the free press and voluntary associations have performed this critical function in our pluralistic society. However, the difficulty of remaining technically well informed and the tendency to assume technological and related economic imperatives in many areas of social interest from national security to personal health and well-being has discouraged creative criticism. Nevertheless, the record of the arms control and environmental movements suggests that associations espousing different values, goals, and interests are coming to play relatively more constructive and important roles in policy-oriented studies. Future policy issues are being anticipated in ways that may forestall unnecessary, but possibly irrevocable, commitments to the application of one or another technological solution to a social problem made in ignorance of foreseeable consequences. Anticipatory studies of this kind give substance to the patterns of application labeled C in Figure I.

Such efforts tend to be institutionally incompatible with efforts dedicated to the development and application of technologies or to the advance of the sciences in terms of direct applications. Legislative mandates for environmental and technological impact assessments are a first step in the creation of legitimate, independent institutional support for policy-oriented studies. Viable intellectual institutions with the independence and capacity to make critical policy assessments in these terms have yet to be developed, however.

Expert Generalists.

Potentially, policy-oriented studies will contribute significantly to general higher education. As such studies help expand policy choices, they can be used to train individuals to function better as the integrating devices that alone can make the kinds of choices which distinguish a political determination of a public policy from a calculated solution to a defined problem. Collective efforts and routinized procedures cannot substitute for the human being as an integrating and judgmental device expressing unique combinations of interests, values, and circumstances.

Policy-oriented studies are not, therefore, just the province of the scientific specialist or a new profession, no matter how helpful such specialization might be. Rather, as the quality of policy-oriented studies improves, educated competence as expert generalists is likely to emerge among scientists and other professionals, as well as among leaders in public affairs. It is likely to be an adjunct to any one of a number of different expert specializations rather than a free standing capability. The pervasive need for expert generalist competences creates a formidable, but by no means insurmountable challenge to our educational institutions and to educated leadership.

Self-correcting Aspects of Policy-oriented Studies.

Because inquiries aimed at policy-oriented studies must take account of the current and possible future public policy environments and of scientific knowledge, they require a capacity for timely self correction beyond that typically involved in either the decision-making arena, where the emphasis is on immediate accountability with less regard for truths or unattained possibilities, or the world of science where the emphasis is on the endless search for truths with relatively little accountability for the consequences.

The emergence of policy-oriented studies as a form of intellectual inquiry thus enhances the possibility that the sciences, and most particularly the social and humane sciences, may not only be applied more fully to public problems, but that such studies may also contribute to improvements in the quality and effectiveness of policy-oriented studies themselves. In this way the disciplines dedicated to studies of man and society and related questions of values, interests, and goals can be expected to contribute to the processes of applying knowledge, even though they may have relatively less to offer to the specific solution or resolution of a public problem.

These five observations indicate some of the implications which the emerging structure of interdisciplinary efforts to apply science to public problems may have on the uses of knowledge, on the science enterprise, on our intellectual institutions, on the education of individuals, and on the capacity of our society to manage interactions between knowledge and public affairs in a more systematic, holistic, and self-correcting fashion.

In the history of mankind and of our civilization, the major new resources available on Earth have been the accumulated store of potentially useful knowledge about nature and society and the continually creative human imagination fed by this knowledge. Now, knowledge of how to generate scientific knowledge virtually at will and on a mass production basis has made it possible to exploit material resources without the practical limits of the past. Knowledge is no longer the limited factor it once was. In the future any limiting factors will have to be based on specific knowledge of how and when to use scientific knowledge in this fashion and when not to do so.

Thus far such knowledge of how best to use resources and to husband them has been demonstrably inadequate, considering the total aspirations of humankind. The structure of interdisciplinary inquiry discussed in this paper suggests that there is a discernible evolution in the structure of knowledge and its applications that can help overcome this inadequacy. More can be done to accelerate the process. The key is the emergence of policy-oriented studies based on an increased capacity to identify and take account of critically important policy-relevant scientific knowledge and technical facts, and the use of such studies in the processes of making and implementing public policies.

> "The dimensions of society's problems seldom coincide with the boundaries of academic disciplines or any combination of them. ... nevertheless, there is now an emerging structure to knowledge in action that may help overcome these limitations."
>
> *Christopher Wright*

III.

WORKSHOP SET 1:

ASSESSING PRESENT FORMS

"We're coming to an age where we begin to question the real value of the disciplines and their associated reductionism, including the paradigm fads that drive a discipline so strongly."

C. West Churchman

" ... holism is a fine concept or ideal, but won't it simply immobolize anyone who tries to take it as an operating principle ...?"

Gordon Enk

A.

CONCEPTUAL DIFFICULTIES IN PROBLEM-ORIENTED RESEARCH:

FORMULATING THE "HOLISTIC" QUESTION*

Charles P. Wolf
Frederick J. Rossini

In his background paper for Workshop A, Wolf posed the holistic question as "ill-defined," one about which there is considerable uncertainty as to desired outcomes and the steps necessary to achieve them. Policy questions, e.g., energy policy, are typically of this order, and methodologies for policy analysis such as technology assessment share in this ambiguity. Although scientifically interesting just because of these complexities, 1/ ill-defined problems fall outside the boundaries of "normal science" as Thomas Kuhn (1970) defines it. Rather they occur at preparadigmatic stages of scientific development or at points of "paradigm crisis." 2/ While these are fit occasions for holistic questioning, they violate the criterion of scientific "goodness" that directs attention and concentrates resources on tractable problems -- those ripe for solution. As well as being refractory, problems of this order are characterized by their interdisciplinarity. They appear to call for a different order of cognitive skills than those of the dominant (empiricist-reductionist) paradigm, skills we presently cannot exercise with systematic mastery. The holistic question thus remains an open one.

DISCUSSION

With this as a starting point, the discussion moved off in all directions. This summary offers some reflections on the talking points made as well as reporting their substance.

Approaches to Holistic Questioning

In addressing the holistic question we can proceed along several lines. Refinements on the opening question take such forms as these:

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* Charles P. Wolf (Chair), Frederick J. Rossini (Reporter), Clair Blong, Rosemary Chalk, C. West Churchman, Harvey Dixon, Robert Knapp, Genevieve Knezo, Ann Maney, Richard Rettig, Robert Rich, Arthur Weiner, and Carol Weiss.

- What is our experience of situations to which holistic questions may be appropriately directed or in which they are effectively engaged?

- How do holistic questions arise? In what contexts, circumstances and cases? What kinds of problems require a holistic approach?

- What do we understand by the "holistic question"? How do we know we have the question right?

- What is the process -- institutional as well as intellectual -- by which holistic questioning can be carried out?

- How can holistic questions be bounded so as to appear manageable; especially, how can conceptual boundaries be drawn in a nonarbitrary fashion?

- Surely we are dealing with analyzed wholes for thinking about them to commence. What is the proper mode of "holistic analysis" and can it be performed consistently, reliably, and in such a manner as to retain or regain wholeness?

- Who is asking the holistic question? What are their purposes in asking and their uses for answers? For example, how can holistic thinking enter into the decision-making process?

- What are some exemplars to holistic questions and what is their demonstrated utility?

- Can holistic thinking be learned, and if so, what can be done to raise the learning curve and apply the results?

The discussion that followed raised, but failed to resolve, a number of these questions.

Why Holism?

Though it took some time surfacing throughout the conference, an underlying assumption of participants seemed to be that science as a social institution is experiencing a "crisis of confidence" not unlike those besetting other institutions (Moynihan, 1967). What is more disturbing is that more is expected of science -- perhaps too much. In 1948 George A. Lundberg asked, "Can Science Save Us?" His answer was affirmative, but this proposition of "science as salvation" appears increasingly dubious as social problems persist and intensify amid the aggrandizement of scientific institutions. 3/ There is growing dissatisfaction with scientific performance in societal problem-solving and with its competence for serving human purposes. These signs point to a paradigm crisis in "normal science" and motivate a desire to replace the dominant paradigm with one sufficiently robust to meet the challenges of social crisis. That is what our discussion was about -- finding new ways of doing science that meet the conditions of our existence, without sacrificing their scientific quality. This dilemma is effectively posed by Mitroff and Featheringham (1974: 393): "In the end the question facing science may be, What would we rather have, precise answers to questions loosely if not poorly conceived or significantly less prescise answers to questions better conceived?"

Can Holism Save Us?

Given these perceived failures in normal science, holism presents itself as a logical candidate for paradigm replacement. Is scientific activity overspecialized? Become interdisciplinary. Is scientific imagination constricted? Become comprehensive. But *is* a new scientific paradigm really justified for meeting social needs? *Is* the dominant paradigm, with all its well-advertised successes, really threatened with institutional breakdown? And, *Is* a holistic paradigm for scientific research now emerging?

What is Holism?

The term "holism" is attributed to Jan Smuts in his 1926 book, *Holism and Evolution* (Ansbacher 1961: 142). Its biological orientation has broadened to encompass psychology 4/ and, indeed, general science (Bertalanffy, 1968). As a movement of scientific thought, holism was propelled by the Unity of Science "movement" of the 1930's and later by the General Systems "movement." Central to this approach is the concept of "system," and *whole* systems at that. 5/ Hardin (1972: 38) draws the implication in ecological context that "We can never do merely one thing." Providing a solution at one point in the system creates a problem at another. This is what Amory Lovins calls the Principle of Interrelatedness, which he views as underlying "the incredible tangle of human problems." The task for holistic thinking then is to translate problems identified and defined in this larger social context into proper scientific questions. According to Mitroff and Blankenship (1973: 351), a methodologically complete holistic approach would involve the sociology of knowledge, the philosophy of science, ethics, law, epistemology, and systems science. It is this conceptual apparatus which the holistic approach must bring to bear on social diagnosis and melioration.

"The Trans-Scientific"

Karl Popper (1957: 74) contends that "wholes" in this sense can never be the object of scientific inquiry. The scientific "revolution" impending or intended aims at precisely that, however; that is its revolutionary promise. In the revolutionary process it will necessarily redefine and reconstruct the scientific enterprise itself. Problems are given and solutions are sought not by the scientific community alone but by the larger societal community of which it is a leading part. The paradigm "fit" of mapping scientific knowledge onto the structure of societal revolutions will require adjustment in the former as much as in the latter. This course steers dangerously close to the "scientific socialism" of Marxism-Leninism, in which scientific integrity has been compromised and the autonomy of science has been subordinated to the rank of "party sciences." But this lamentable precedent is a perversion of the holistic revolution which strives to redress imbalance in the rise of science in relation to society. 6/ Nevertheless, the Marxist science paradigm is correct in recognizing the dialectic of science and society and that "politicization" of science cannot be avoided. Similarly, Alvin Weinberg (1972) has argued that, for problems attended by great uncertainty or having strong political overtones, adequate solutions cannot be supplied by science alone. Solving such problems, he considers, is as much a political activity as a scientific one. It follows that to crystalize the holistic paradigm involves the creation of trans-scientific communities and settings.

Trans-Scientific Communities

Does holistic thinking require the evolution (or revolution) of a new paradigm within the scientific community? It might be fairer to say it requires the evolution of a new scientific community, one which incorporates all parties at interest and acknowledges all sides of the complex relationships involved. Science can no longer restrict itself only to the "scientific" formulation of research questions. The decision makers' viewpoints must participate directly in the research process, not as filtered through the scientists' perspectives. The new conceptualizations for research must deal with the needs of both the scientific community and decision makers, and with those of the public as well. The problem of holistic knowledge includes understanding the human systems in which it is created, transmitted, and applied.

The Value Question

By inducting new members into the trans-scientific community we necessarily incorporate their values and perceptions as well. Contrary to the value exclusion practiced under the dominant paradigm in the names of "objectivity" and "scientific neutrality," value inclusion is integral to the holistic approach. Morality, ideology, and intuition thus claim a legitimate place in trans-scientific inquiry. Since personal and social truths, including values, are based upon diverse individual and collective experience and the interpretations placed upon it, we arrive at a paradox of the unity of knowledge and the plurality of truth. In the holistic paradigm there are many complementary approaches to knowing. Because plural, the intellectual task is more difficult than simply changing the conceptual system of one or another specialist group. The "republic of science" becomes a "democracy of theories." Moreover, there is a kind of reflexiveness in the relation between knowers and ways of knowing through the holistic paradigm. What intellectual clarification and coherence are possible in the midst of this diversity?

The Uses of Holism

If the scientific truths a holistic approach might discover are relative and not absolute, their applications must likewise be contextualized. Holistic questions are not an end in themselves but a means for examining the hidden assumptions and complex interactions that elude partial approaches and analyses. Decision-makers are led into errors of judgment and action by the fragmentary condition of knowledge on which their decisions are based. Can holistic thinking become action-oriented in a way which would prove helpful to the problem-solver or decision-maker? Not, it would seem, without venturing into the institutional arenas of policy formation and decision. The scientific division of labor is not such as to make self-administering. But effecting an "entry into politics" encounters the familiar institutional barriers of limited authorizations and jurisdictions. Holistic thinking is inhibited and constrained by the institutional configurations of departmentalism and compartmentalism, specialism, and sectarianism. To articulate with the policy decision system requires a deeper understanding of "user needs."

The Users of Holism

Experience with decision-makers in federal, state, and local agencies suggests that in cases such as mental health the use of social science research information is less for problem-solving than for background knowledge, agenda setting and problem formulation. Policy-makers are not moved to acting on holistic knowledge because of the large-scale, perhaps utopian, changes implied and the tendency to treat as policy variables forces such as population growth that are largely beyond their control. Policy is rather made through a series of small-scale changes -- "disjointed incrementalism" -- and the holistic approach does not seem to mesh well with today's decision-makers' information needs. Even broad policy issues may be fractionated into narrow decision contexts and choices. At a minimum, there is a considerable translation problem between holistic approaches and the particular situations in which they must operate. How can the closure and certainty that policy-makers desire be secured by following the holistic approach? More important, how can situations and conditions recognized as problematic be changed without changing the systems that generate them?

SUMMARY

The preceeding points were some things thought about in workshop discussion. As with the background paper, it better illustrated than alleviated the conceptual difficulties in holistic thinking. If there was anything like convergence in such a free-form exchange, it seemed to point more in the direction of describing symptoms of scientific malaise than of prescribing the basic intellectual framework for remedying them. If any recommendation was forthcoming, it seemed to favor continuing the dialogue begun here. The holistic question continues to remain open, but perhaps we raise it now with fresh urgency and clearer insight.

FOOTNOTES

1/ As Ronald Howard of Stanford University defines the problem space, there are three main components of complexity: number of variables, degree of uncertainty and time factor. In the "worst case" of holistic questions, variables are numerous, probabilistic, and dynamic.

2/ Masterman (1970: 61-65) catalogues no fewer than 21 different meanings of "paradigm" in Kuhn's first (1962) edition of The Structure of Scientific Revolutions. She groups them in three main classes: metaphysical, sociological, and construct paradigms. In terms of a "pattern language," we can speak of a paradigm as a "main pattern" of intellectual activity. The preparadigmatic stage is then roughly analogous to "pattern recognition" and paradigm crises to "pattern breaks."

3/ A condition resembling the problem of evil and the ascendence of religious institutions historically.

4/ For example, "It is characteristic of the holistic approach that it views the human being as an organized unity and seeks to understand various phenomena of human behavior in terms of the underlying organization. Thus the concept basic for holistic explanation is that of organization or integration (Angyal 1948: 178).

5/ Mitroff and Blankenship (1973: 339) define "whole systems" as those where "the behavior (performance) of each of the components cannot be measured or evaluated apart from the whole system of which they are a part." These are contrasted with "atomistic systems" in which the opposite conditions and procedures apply.

6/ Wittfogel (1963: iii) calls for a "macro-analytic revolution" -- the need for "big-structures concepts" to wage ideological competition against Marxism. Apart from the Cold War polemics in which he couched his phrase, what seems distinctive of current developments is a demand for empirical precision as well as intellectual substance. "Holistic *and* quantitative" might be its manifesto.

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B.

MOTIVATION AND REWARD STRUCTURES: WHAT ARE THE INCENTIVES AND RISKS IN DOING PROBLEM-ORIENTED RESEARCH?*

Ronald Corwin
Sherry Arnstein

Three questions guided the workshop in its discussion. This summary is organized around those questions--the bullets indicate the group's responses. Six recommendations resulted from the discussion.

DISCUSSION

1. What Are The Differences Between Multidisciplinary And Interdisciplinary Approaches To Problem-Oriented Research Projects? Are There Different Risks And Incentives For Each?

   - Both multidisciplinarity and interdisciplinarity are viewed as means and not an end to problem-oriented research. While interdisciplinarity is harder to achieve, it will increasingly be required as scientists become more involved in studying complex societal problems. They involve a different understanding of how the problem is to be defined and how the research is structured. Multidisciplinary research tends to fragment a problem into different parts which can then be studied separately by representatives from the various disciplines involved. These separate studies are then assembled into a final report consisting of discrete chapters contributed by the various team members involved and frequently includes some after the fact overview and final chapter(s) grafted on by the principal investigator. Interdisciplinary research is more team-oriented, and its primary distinguishing features

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* Ronald Corwin (Chair), Sherry Arnstein (Reporter), Kenneth Beasley, Donald Gerwin, Lowell Hattery, Kenneth Heathington, Donald Michael, John McKinney, and Vernon Root.

2. Do These Incentives And Risks Vary Within Different Organizational Environments? Are There Techniques Of Overcoming The Risks And Enhancing The Incentives?

- The incentives and risks for engaging in problem-oriented research vary not only among but within discrete organizations. Land grant colleges, for example, have a long-established norm for applied research while traditional colleges have more generally favored basic knowledge development. Moreover, although much attention has been directed toward the problems of the universities in generating good interdisciplinary research efforts, there is some evidence that private research firms also experience difficulty in mobilizing interdisciplinary teams. These difficulties within private research groups are seldom talked about openly or reported in the literature because interdisciplinary studies are a major marketing strategy used by the firms which are, in fact, most frequently producing multidisciplinary studies.

  Certain departments within a university are much more prone to encourage interdisciplinary work than others. Since interdisciplinary efforts emphasize a shared approach toward studying a problem, only those individuals who enjoy this sort of exchange would be likely to seek out this kind of work. In the laissez-faire environment of the university, however, the volunteers for interdisciplinary teams are in the minority.

  An important factor affecting a university department's attitude toward interdisciplinary research centers upon how that activity might affect the departmental budget. If a faculty member participates half-time in an interdisciplinary project and his or her department does not retain full authority over that member's salary and some percentage of the overhead allotted to contract awards, then the department perceives a negative influence upon its general resources, both in personnel time and dollars.

  Another factor affecting the departmental attitude is the promotion or tenure review of a faculty member engaged in interdisciplinary research. If department heads and faculty do not directly review work performed outside of the department, then that effort may be perceived as "lost" or "wasted" time for the faculty member under review. This perception could are a shared definition of the problem that permeates the total research process and a final product which blends the various contributions so that neither the identity nor the disciplinary background of the individual authors can be determined. Instead of dividing the tasks along traditional disciplinary lines, the research team jointly structures the problem and draws on appropriate tools and techniques from each of their fields, sometimes inventing new methods to cope with those questions which inevitably will cross disciplinary lines.

These two approaches may be characterized as the "coexistence" model where they cooperate without conceptual integration and the "collaboration" model where they jointly specify the problem and jointly select techniques to work on the solution. Multidisciplinary research offers greater incentive in terms of its potential for evaluating each researcher's contribution to the final product. The researcher is able to gain more recognition for his or her study since the contribution represents a disciplinary effort. Departmental endorsement of multidisciplinary projects is usually much easier to obtain as the researcher in most cases is not required to be absent from the department location, and the contract dollars for the study can be directly channeled through the departments involved. Interdisciplinary research, however, offers more risk in this area by nature of its consolidated or joint research process and product. The interdisciplinary framework tends to wash out the identity of the contributors and, therefore, it is very difficult to evaluate each individual or disciplinary effort.

In some cases, however, the incentives for interdisciplinary research outweigh this risk because the research offers a more realistic approach to the problem and its results thus tend to be more useful. The ability to interact fully with other disciplines on a problem-oriented task also presents a motivational incentive for those researchers who are interested in learning from other disciplines. Yet the hierarchy or level of sophistication of the methodologies involved in an interdisciplinary effort also affects the team effort and mode of operation. The "meshing" process thus runs the risk of one discipline dominating the others. The economist's input-output model, for example, when presented to a team can appear so powerful that the team converges around the model with little recognition of how much data and information are excluded if the model is adopted as the driving technique of the study.

be balanced by evaluation from external reference groups. However, in many cases where interdisciplinary efforts are involved, the department does not fully recognize or endorse either the journals or organizations which might serve as this balancing review agent. Publication in interdisciplinary journals, for example, does not satisfy the "publish or perish" requirement of many departments which will withhold promotions and tenure unless the faculty member has published in the traditional professional disciplinary journals.

It should be noted, however, that the hiring and promotion value system of the university are coming under close scrutiny by the departments and faculty themselves as well as the administrators, students, and, in some cases, outside funding sources. There is a beginning groundswell of feeling that the existing peer review system reinforces the status quo and all too often restricts the departments from innovating or experimenting with alternative organizational approaches (such as short-term projects between departments or colleges, team efforts, and recruitment of outside talent for limited duration of studies).

Some academics and program managers perceive extended interdisciplinary research activities as a serious threat to a researcher's disciplinary knowledge base. If the researcher consistently works on problem-oriented tasks and does not "re-tool" within his or her discipline, the methodologies and insights that are brought to bear on the research project may grow out-of-date with the more recent addition of methodology and theory within the disciplinary knowledge base. There is a perceived trade-off here between using a given level of knowledge (which may be a year or two out-of-date) in order to do policy-relevant research, and developing up-to-the-minute research within a discipline which may have no policy relevance at all. Although these different choices may appeal to different kinds of individuals, the university reward system often encourages the latter and thus inhibits the interdisciplinary or policy-relevant research process. It is argued by some that the university has no business engaging in this kind of research at all. Rather, the role of the university is indeed to build the knowledge base which can be applied elsewhere to real world problems.

3. What Academic Trends Seem To Be Affecting These Risks And Incentives For Interdisciplinary Research?

- Some traditional disciplines have prepared a segment of their population for working in a diverse range of organizations, and thus these various career choices were viewed as an "acceptable" or "legitimate" use of the disciplinary knowledge. Others -- such as economics or sociology -- were strongly oriented toward preparing students for university careers and are just beginning to make this switch toward legitimizing diverse careers in nonacademic settings. This switch, caused for the most part by the declining academic market, makes it unreasonable for current university faculty to continue to try to create replicas of themselves. Eventually it may create a new value system within the students as to which kinds of knowledge are more useful and relevant for realistic career selection and preparation.

  Some professional schools -- in business administration and social work, for example -- have attempted to prepare a student for a wide range of roles and tasks which often require gathering knowledge and information from diverse sources. Yet the training for these students often represents a strictly multidisciplinary approach; that is, the student is provided lectures and reading lists on the different parts of a subject but is not offered the synthesizing or holistic analysis which address the problem-solving process. It is left solely to the student (or eventually to the business manager or social worker) to put the various components of data and information together. Nevertheless, the professional schools may be more conducive to future interdisciplinarity than the discipline departments. They are already problem-oriented; they are conceptually prescriptive and normative in their approach; they have a map of the outside world to which the curriculum is related; they already have numerous disciplines appointed to the faculty; and they seem to be increasing the number of electives offered to students.

  Blends of traditional disciplines are beginning to emerge -- such as social psychology, physical chemistry, etc. -- which are viewed as a further refining of the reductionist process rather than a trend toward interdisciplinarity. These blends, coupled with rapid growth and new discoveries, often represent a way for rising faculty members to gain status and prestige within a fairly narrow time span.

Interdisciplinary problem-oriented research is receiving more attention now than it did ten years ago and, like most conceptual ideas, it takes time to achieve what Donald Schoen terms "ideas in currency."

RECOMMENDATIONS

While Workshop B did not attempt to reach consensus on recommendations for reducing risk and increasing incentives for problem-oriented research, various action proposals were discussed. These included the following:

- The AAAS could play a leadership role in fostering problem-oriented research. This leadership role might include such actions as sponsoring symposia, inviting original policy science articles in Science magazine, and giving recognition to interdisciplinary journals by reprinting selected policy articles.
- The AAAS could consider changing its name to The American Association for the Advancement of Society Through Science to indicate its endorsement and legitimization of problem-oriented research.
- Universities interested in fostering interdisciplinary problem-oriented studies could make it clear that they support this kind of research by introducing a fundamentally different approach to tenure and promotion reviews for those faculty members who do participate in interdisciplinary programs. For example, they could require departments to offer to involved faculty members such incentives as reduced publication requirements and acceptance of publication in interdisciplinary journals.
- Universities could encourage departmental participation in problem-oriented research by providing extra overhead dollars for those departments which are willing to engage in interdisciplinary studies.
- Departments could encourage involved faculty by providing such incentives as reduced teaching load, reduced publication requirements, extra secretarial support, and extra sabbatical time.

- Universities interested in fostering interdisciplinary research might consider introduction of the innovative incentives employed by the Transportation Center at the University of Tennessee. These include:

  A line item support in the University budget for the Institute;

  Salary plus overhead on a staff of 110 which goes to the Institute;

  Reimbursement to the department for the salary of a faculty member plus 50 percent overhead;

  Faculty participation in Institute studies is recognized by the department as one out of fifteen evaluation items considered for promotion and tenure reviews.

> " ... natural rejection mechanisms are triggered in universities when such interdisciplinary problem-oriented organizations are created. Strong support from an administration, lots of money, and a strong leader may serve as an immunosuppresent, but a history of such organizational efforts would have to be entitled 'A History of Failure'."
>
> *Don Kash*

C.

PROBLEM-ORIENTED RESEARCH PROJECTS

LEADERSHIP, MANAGEMENT, COMMUNICATION FACTORS*

Leslie Rugg
Raymond Woodrow

GENERAL POINTS

In the discussions of this working group, it was understood that interdisciplinary research meant interdisciplinary, public problem-oriented policy research. Stimulus statements and questions for this group included the following:

> ". . . quality interdisciplinary research is performed in spite of the traditional university environment, not because of it."1/
>
> What are the differences between management of interdisciplinary research and traditional research?
>
> When does a university commit "hard" money to interdisciplinary research rather than relying solely on outside sources?

The group recognized that strictly speaking, interdisciplinary research *per se* does not equate with problem-oriented research. For example, problem-oriented research in fields like engineering and agriculture is not interdisciplinary. At the same time, there is much interdisciplinary research that is not problem-oriented, such as environmental studies, international studies, and urban studies.

Two key areas of emphasis emerged from the discussions. Better ways must be found for providing incentives to university faculty to participate in long-term interdisciplinary research. Traditional university reward structures, which are almost universally tied to discipline-oriented departments, do not provide this motivation. Better ways must also be found to encourage individual faculty member participation in the team effort required in true interdisciplinary research.

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* Leslie Rugg (Chair), Raymond Woodrow (Reporter), Vaughn Blankenship, Harold Chestnut, Bernard Cohen, Walter Hahn, Don Kash, Ann Macaluso, David Rose, David Schuelke, and Christopher Wright.

The project director is critically important to the success of the operation and utilization of interdisciplinary research. Leadership and the ability to promote the project and communicate its results both internally and externally may be more important attributes of a potential director relative to a successful outcome, than is outstanding scientific competence.

The discussion of the workshop group can be organized as responses to three broad questions about interdisciplinary, public problem-oriented research.

MANAGEMENT CONSIDERATIONS

What are the important management considerations regarding interdisciplinary research?

Interdisciplinary research, as compared to multidisciplinary research, requires consistent working, problem-focused interaction among team members to achieve (1) a shared understanding of the problem definition, (2) the techniques available for studying the problem, and (3) the information each discipline can introduce into the research process. This interaction is especially important in the beginning or start-up phase of the project as team members must learn how to communicate with each other despite disciplinary jargon obstacles and to develop a respect for the contributions each discipline may make.

Since an important part of the research process is the problem definition task, considerable attention must be given to the planning stage of the research project and the selection of disciplines which should be involved in the study. However, it is this planning stage which is most difficult to fund and justify and which most often falls to low priority in the time schedule of many project managers who are committed to the projects in process.

The dissemination and utilization phase of interdisciplinary research involves not only publication and distribution of a report, but also interaction by the team members and the project director with those persons who are identified as users or potential decision-makers in those areas under study in the project. The decision-making process usually is comprised of many people and extends over a wide range of decision points. If the research product is to be effective then the results of the research must be brought to the attention of those persons most capable of implementing the research findings. Often, even in first rate interdisciplinary policy research, inadequate time and money is allocated for this phase. Active dissemination of the results is further discouraged by the lack of status within an academic community associated with such activities. The academic emphasis is placed more solely on objectivity and excellence, rather than also on potential usefulness and actual utilization of research.

The management of interdisciplinary research requires an administrative "flexibility" relatively unique within the more traditional environment of the university. The ability to offer such amenities as secretarial support, editorial assistance, travel and professional meeting subsidies is an important factor in this concept of flexibility. The assurance that such amenities can be given are some of the extra points necessary to provide necessary additional incentives to faculty members to engage in non-traditional action-oriented research.

ORGANIZATIONAL DESIGN CONSIDERATIONS

How necessary is organizational design to the management of interdisciplinary projects? Is it enough just to have a group of good researchers working on a project together in an interdisciplinary mode?

The role of the project director is critically important to the management of interdisciplinary research. He must skillfully exercise his entrepreneurial, conceptual, managerial, and communication talents, as well as a continuing sensitivity to the needs of the team members. Yet often the project director is managing a team of researchers who are assigned to the project on only a half- or quarter-time basis. The loyalties and energies of these researchers are thus divided between the project and the departments or divisions which they represent within the project. A decision must be made as to whether the director and team wish to concentrate only on a short-term projects or to develop a long-term capability within the selected problem area. If they choose the latter, an organizational basis will be required which can offer the same rewards and incentives to the team members that they receive from their own departments.

An important consideration affecting the management of problem-oriented research projects is the lack of long-term support for programs which could develop the capability for doing research in specific problem areas. Unless a university administration is willing to invest a sizeable amount of resources to provide program support, an action which is increasingly improbable in a time of rising costs within the university, the team of researchers wishing to do problem-oriented, interdisciplinary research must rely on project funding from outside sources, primarily the Federal Government. The organizational design must provide some flexibility for absorbing the transfers of personnel and resources, which are involved in multiproject managements. The project manager therefore requires strong administrative support to minimize the energy and time involved in starting-up and winding-down projects funded from separate sources.

A research group funded through short-term project awards rather than long-term program support does not have the opportunity to develop what might be called "the survival instinct." At the initiation of a short-term project, this "instinct" is missing, and the project does not develop the organizational base required to survive in a larger competitive interorganizational environment. Instead, the project team goes straight for the product (research results) and then disbands. Some of the people move on to other short-term projects and the process begins again. The management approach required in the absence of the survival instinct thus tends to emphasize research responsive to the immediate decision-maker's or other client's needs. It discourages long-term approaches which would require team interaction with users or implementers of the research beyond the duration of the project life itself.

LEADERSHIP CONSIDERATIONS

What types of persons are most likely to become involved in interdisciplinary research within the university environment? How is leadership developed within the project team?

Many interdisciplinary research projects receive their main impetus from a single or group of senior individuals who have already established their academic reputations within a disciplinary field and who have then decided to move into a problem-oriented research area. These senior men and women have the capability of attracting a group of younger faculty members who desire to have the opportunity of both working with the senior person and doing research in a problem-oriented area rather than focusing on a specific disciplinary approach. As this younger faculty becomes more exposed to the university reward systems, the professional risks involved in spending a great amount of time in problem-oriented research become more apparent. Without some form of built-in organizational reward system, which equals the departmental incentives, the younger faculty must choose between working in the problem-oriented area or returning to more traditional disciplinary research in order to develop their own university status. The reputation or endorsement of the senior team is often not sufficient to gain departmental recognition of the value of interdisciplinary research.

This "major founders" approach to interdisciplinary research programs is much more entrenched at some universities than others; in most cases, it is those universities which have stronger departmental ties that are more likely to develop this approach toward interdisciplinary research as opposed to establishing an organizational basis for program support.

Different characteristics are required for the leadership of the interdisciplinary research project through its separate phases, and it may be useful to change leadership at different phases to more correctly match these characteristics. The start-up phase needs a manager who can both attract individual researchers and who can provide an environment conducive to breaking down barriers and analyzing the different aspects of the research problem through a team approach. After the investigation has been completed and the report is beginning to develop, it may be useful to bring both a fresh perspective to the research task and to synthesize the separate findings in a more interdisciplinary and interactive fashion. The project leader at this stage also needs to be an aggressive promoter of the research product -- interacting frequently and directly with potential users -- if its results are to be incorporated in the decision-making process.

FINAL CONSIDERATIONS

The workshop group did not have adequate time to address fully whether these questions are, in fact, the best ones to ask. The need for interdisciplinary policy research taking place in different settings was generally taken as a given. It was recognized that the case supporting the need requires additional analysis and, presuming that such analysis strengthens the view that the need is great, additional advocacy of effective interdisciplinary policy research.

Another recognized need is for a better understanding and a greater general awareness of the inherent rejection mechanisms within universities that inhibit interdisciplinary research.

FOOTNOTES

1/ Nilles, Jack M. "Interdisciplinary Research Management in the University Environment," Journal of the Society of Research Administrators, (Spring, 1975), p. 9.

D.

ALTERNATIVE ORGANIZATIONAL DESIGNS TO MEET SOCIAL NEEDS*

Arie Lewin
Ian Mitroff

The workshop group had as a stimulus and guide for discussion a brief statement prepared by its chairman. The statement focussed on attributes and characteristics of the policy sciences. It stated that:

> "As a result of our deliberations we may conclude that the answer is not to be found in structural changes of our existing research and problem-solving organizations. It is the objective of this workshop, however, to explore alternative organizational designs ranging from the conception of radical new organizational designs to the adaptation and redesign of our current institutions as called for, and to explore their organizational and political feasibility."

The workshop group agreed that it is the professions in our society which are charged with solving real-world, systemic, ill-structured problems. Given this, they felt that the professions needed more support at the university plus societal levels. Allen Rosenstein suggested the establishment of a National Foundation for the Professions which would play a role similar to that played by NSF for science. (He has already helped to introduce a bill, to that effect in the Congress.) The main idea here is that we vitally need different institutions operating from a different perspective to support holistic, interdisciplinary, public problem-solving.

Gerald Gordon and others suggested we now know enough to design an institution to support holistic research. A key variable seems to be the psychology of the research administrator. If you want effective interdisciplinary research, don't select a narrow disciplinarian as a leader.

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* Arie Lewin (Chair), Ian Mitroff (Reporter), Nathan Caplan, Kan Chen, Robert Cutler, Gerald Gordon, Paula Gordon, William Newel, Allen Rosenstein, Saleem Shah, James Taylor, and John Waring.

The group's discussion is organized under four summary questions developed within the workshop discussion.

DIFFICULTIES OF PROBLEM-ORIENTED RESEARCH

What are some of the difficulties involved in establishing public problem-oriented research programs within traditional organizational environments? How can these difficulties be reduced?

A basic weakness in interdisciplinary programs within university organizational structures is the disciplinary model upon which they are based. The faculty who staff these programs have all been trained in disciplinary environments. In interdisciplinary efforts they are expected to interact within these innovative programs in a completely new way. This is an unrealistic expectation. Such interaction must be taught and nurtured in appropriate, interested participants. Furthermore, problem-oriented research is often viewed as not a wholly legitimate undertaking for a university.

The role of the professional, as an agent of change in problem-solving activities, has been grossly underestimated within the university community. The National Science Foundation, as the major source of funds for university research and development in recent years, may be warping the academic environment, emphasizing excellence and scholarly criteria too heavily over other professional, action-oriented criteria. The status of the professional schools need to be boosted within the university. NSF is not the proper model for the professional schools, although it may be a good model for the science schools.

In addition to the lack of outside funding support, there is also a lack of top-level administrative support within the university structure. The Vice-Chancellor for Research and Development provides one channel for this support, but there is no readily identifiable office to which the professional schools can go for administrative assistance in problem-oriented, rather than discipline-oriented, activities. We need to train people in this mode professionally, not strictly from a one-dimensional view. This requires multiprofessional education programs in the university, supported internally and externally. Multidisciplinary or interdisciplinary programs often fall short in curriculum design, because they receive little or no nourishment from the discipline-oriented departments or top-level administrative offices.

Basic research on the methodology of real world problem-solving falls outside the scope of most government R&D programs (including RANN), and also outside the disciplines. It is extremely difficult to generate support and find funding for this kind of research.

There may be a need for something like a National Foundation for the Professions which would offer support for action-oriented, multiprofessional policy research. This research currently finds little encouragement elsewhere.

ORGANIZATIONAL DESIGN LESSONS

What organizational designs have attempted to deal with these difficulties? What lessons are to be learned from these design experiences?

Organizational design has only recently begun to emphasize the design of these institutions. Reliance previously has been placed on structure rather than the process of communication and action between structures. Avoidance patterns are a common force for designing organizations in the absence of control mechanisms within the design.

RAND and NIH may be viewed as prototype problem-oriented organizations. The original design and congressional mandate of NIH was intended to establish an organization which would focus on applied, mission-oriented research. Its purpose was to facilitate the work of the medical profession rather than support individual disciplinary research. Yet today NIH views itself as a discipline-oriented, rather than a problem-oriented organization, and the reason for this change in direction can be attributed to the power in the discipline-based advisory panels which review grant applications on the basis of scientific excellence rather than their relevance to human health.

The capability to perform problem-oriented research requires a substantially long lead time. An institutional memory of how to effectively develop this capability is not developed when a program relies solely on project by project funding. If the universities are to develop this capability, they must also incorporate it into their education and training programs. They should be encouraged to educate students for multiprofessional problem-definition tasks, emphasizing the development of a professional methodology for dealing with ill-defined, ambiguous questions. There is some doubt whether the university can undertake this task.

PROBLEM-ORIENTED VERSUS DISCIPLINARY RESEARCH

What mix between problem-oriented and traditional disciplinary research is desirable? Can one organization provide the proper environment for both kinds of research?

It is not at all certain what the mix or emphasis on types of research should be. Problem-oriented research often focuses on ill-defined or unstructured questions, and operates in the absence of a theoretical base or paradigm. In the Kuhnian sense, problem-oriented research may represent "revolutionary science" and disciplinary research is "normal science." The NSF may be an appropriate source for supporting normal science, but it appears to be ill-suited for the support of revolutionary science because of its bias toward disciplinary research. Therefore an organization would have to seek separate funding sources if it wished to maintain a mix of these research orientations.

Furthermore, interdisciplinary, problem-oriented research is often much more costly than traditional research and thus the mix of management styles could not expect to equally allocate resources between these programs. Given the bias and traditional incentives favoring the discipline-oriented programs, it is questionable whether both can be encouraged and supported within one organization.

The NIMH has some experience in supporting both traditional discipline-oriented research programs and more recently fostering problem-oriented research centers. But even within these centers there is little experience in funding long-term interdisciplinary programs. Rather, the funding for this kind of research must be divided into phases, each of which must be justified and evaluated separately. There is no model in that organizational structure for funding "revolutionary science" which would foster studies without an acceptable theoretical or methodological base.

HOW CAN WE DESIGN NEW INSTITUTIONS?

Do we know how to design organizations which can offer sufficient flexibility and support for those persons who wish to do problem-oriented research? Is it possible to design these organizations within a government agency, university, or private research environment?

From the NIH and NSF experiences with mission-oriented research, it can be concluded that the power of the disciplines has an extremely strong pull upon those persons who wish to engage in problem-oriented research. If an organization is to be designed to do problem-oriented research, it must deal directly with this pervasive power of the disciplinary approach to science and technology.

It may not be enough to simply compensate for the lack of professional support centers in the research environment, although this may be a necessary intermediate step. There may, however, be a real need to learn how to design a holistic organization which would in some way put the researcher in direct communication and interaction with both problem-solvers and the victims of the problems to be solved.

Because of the complex and interactive nature of many social problems, it may be necessary for a problem-oriented research organization to address the whole pattern of a specific problem before offering a solution based on research findings. Yet the methodologies developed in the science disciplines force a certain restriction on the problem-oriented study, so that we can deal only with those parts of the problem which fit the selected methodology. In holistic problem-solving, it is necessary to have these methodologies interact with one another in an organizational design which would not favor one methodology over another. It is extremely difficult to design this process within traditional organizational structures which are often committed to the support of a particular methodology or theorectical base.

Can we create an organizational design for the support of innovative or revolutionary science?: "yes," but only if we can create an organization, or better yet organizations, wherein the key role of the integrator is given conscious consideration and preeminence. By "preeminence" we mean placed in key informational and policy positions so that the integrator can encourage the kind of problem-solving flexibility that is characteristic of innovative or revolutionary science. 1/, 2/

One conclusion is that if you want to have an innovative organization it is a necessary, though not sufficient condition, that you put innovative types in positions of key repsonsibility and authority.

We are obviously not saying that this is the only issue involved in creating a new institutional framework for science. Such a contention would be as naive as it would be absurd. The nature of the issues facing science, not to mention the surrounding political structure plus the internal politics of science, are far too complex to warrant such an assertion. What we are contending is that the psychology underlying the system of science is one of its most important aspects and that the system can no longer afford to be unselfconscious of its own psychology. 3/

FOOTNOTES

1/ Gordon, Gerald, MacEachron, Ann E., Fisher, G. Lawrence, "A Contingency Model for the Design of Problem-Solving Research Programs: A Prospective on Diffusion Research," *Milbank Memorial Fund Quarterly/Health and Society* (Spring, 1974), pp. 185-220.; and Gordon, Gerald and Morse, Edward V., "Creative Potential and Organizational Structure," *Academy of Management Journal*, Vol. 12 (1969), pp. 37-49.

2/ Mitroff, Ian I., "On Managing vs. Administering Science: Towards an Institutionalized Design for Revolutionary Science," in manuscript.

3/ Maslow, Abraham H., *The Psychology of Science*, Harper & Row, New York, 1966.

E.

INSTITUTIONAL ROLES AND LINKAGES IN MORE EFFECTIVELY HELPING TO SOLVE SOCIAL PROBLEMS*

Joel Snow
Daniel Alpert

INTRODUCTION

We started our workshop by talking about ourselves. Each self-history was a statement not only of personal history, but also of personal insights about the issues facing this conference. Our nominal charge was to assess the "roles for and linkages among science institutions in more effectively helping to solve social problems."

(The workshop group had addressed to it three admittedly preliminary questions regarding linkages among so-called science institutions and of science institutions with other problem solving organizations. The members of the workshop questioned the utility of these questions as phrased and, after some discussion, discarded them in favor of the discussion presented here. The original questions and the examination of hidden or misleading assumptions that discussion of the questions uncovered did, however, influence the group's deliberations. Working Notes 1 and 2, appended to this workshop summary, provide interesting and useful background insight into the discussion basis.)

We soon realized we had many communications problems:

Semantics:
- What is a science institution?
- What is a social problem?
- Should we meet social needs?
- Should we meet social goals?

Conflicting values:
- Where do we need linkages?
- Between similar or dissimilar institutions?

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* Joel Snow (Chair), Daniel Alpert (Reporter), Norman Evans, Gordon Enk, Leonard Goodwin, Phil Gustafson, Harry Lambright, and Betty Pickett.

Conflicting models and metaphors:
- What do information flow models implicitly assume?
- Are not knowledge users also in some sense knowledge producers?
- To what extent does knowledge rationally influence the policy process?

Some workshop members were troubled by the model represented by such descriptive notions as "the flow from knowledge producers to knowledge users." The model presumes that so-called decision-makers take action mainly on the basis of academic or "rational knowledge." Several anecdotes suggested that decision-makers often don't want "the facts" and sometimes make seemingly better decisions without them.

Thus the implicit notions that users want or need "the facts" and that these facts may influence user decisions are simplistic. Indeed, we faced the question: Can you use rational means to get people to change beliefs that they didn't arrive at by rational means? One person proposed applied research addressing the question: "What does it take to get people to change their minds if they operate in a conflicting value context?"

WORKSHOP AGREEMENTS

After discussion, we developed several propositions on which we seemed to agree:

1. The key issue involves linkages among individuals -- rather than institutions. Typically, when an interaction takes place, an entrepreneurial activity is involved. Another way of saying this is, "Knowledge does NOT introduce change; people initiate change -- using all kinds of knowledge, of which academic or scientific knowledge is only one."

2. There is a need for entrepreneurs or catalytic brokers, both across departments in the same institutions and across institutions. The entrepreneur negotiates deals between persons from quite different areas of life.

3. We also identified a concern about universities -- that as the financial crunch intensifies, we observe the institutions to become less willing to take risks. And this reduced risk-taking pervades foundations as well as universities. Indeed, the foundations and

the universities are mirror images of each other. It is in this climate of scarce resources that networking -- or sharing resources across institutional lines -- becomes critically important. Transfers of personnel, access to information and data across institutional lines, small group discussions of scientists, discussions between scientists and non-scientists, and common use of facilities must all be part of these networking efforts. These activities therefore will often require decisions by separate administrative offices within the participating institutions and these decisions should be made with full awareness of the implications of the networking efforts.

4. We agreed that there is a need to examine our reward system to encourage entrepreneurs -- to encourage the formation of human networks across institutions and beyond traditional roles.

5. This level of consciousness led to a recommendation for a modest institutional change in AAAS conferences. To encourage the formation of networks, conference formats should be changed to encourage cross talk with kindred souls, to learn more, and to gain social support. Conference designers should break with tradition, accepting a certain amount of risk, and build in unoccupied spaces in the program schedule for unstructured, ad hoc (creative) cross talk.

LEARNING NEEDS AND BARRIERS

Our group came up with three clearly distinguishable learning needs:

Individual learning;

Institutional learning;

Societal learning.

We also recognized certain barriers to such learning -- especially on the part of persons in power. If you are controlling others, telling others what they should do, you may not be in a learning mode. Thus, so-called decision-makers may be incapable of "changing their minds." If you are busy controlling others, you may not be tuned in to the creative flexibility required to foster entrepreneurial and catalytic individuals within your institution.

SCIENCE AND PUBLIC PROBLEM-SOLVING

Certain aspects of problem-solving are heavily value laden; in particular, this is true of the aspect referred to as "stating the problem." It has long been recognized that what constitutes a problem or a solution to some observers doesn't to others. Indeed, one of the most serious critiques of government-initiated social programs in the 50's and 60's involved the "solutions" of health, education,and welfare problems. In each case, the professions perceived a solution in terms of a need for more or better professionals. In the eyes of social workers, public aid could be improved by more or better trained social workers; in the eyes of teachers, education could be improved by more or better trained teachers; in the eyes of lawyers, all public programs could be improved by more or better trained lawyers.

Now, what happens when we researchers (members of the research and development community) arrive on the scene, how do we state the problems and the solutions? -- you guessed it; what societal problems call for is more and better research. It is this kind of uncritical self-serving proposal that has created what some conference attendees have called a crisis of credibility toward all professions as well as scientists.

This crisis of credibility is manifested by a broad challenge to the role of the expert in our society; in every profession we are hearing a demand by citizens for demystification, decentralization,and deinstitutionalization. How can the interested layman arrive at an intelligent judgment about major issues without demanding that the professional experts get out from behind their esoteric jargon? The failure to develop significant problem-oriented research that can be incorporated into the problem-solving process (as it really exists) may have exacerbated this crisis of credibility.

We summarized our deliberations with a renewed emphasis on the role of the individual as a human being -- the individual decision-maker as a human and the individual scientist as a human -- each of us taking actions on the basis of both rational and nonrational considerations, and using intuition and experience -- underated knowledge sources -- as an important guide for belief and for choice.

* * *

WORKING NOTE 1: KNOWLEDGE INSTITUTIONS AND "SOLVING PROBLEMS OF SOCIETY" (Joel A. Snow)

On Research and Societal Problems

It must be stressed at the outset that research doesn't solve problems, but rather provides an improved base of information that may contribute, as part of a lengthy and complicated process, to the solution of a problem. Moreover, speaking of problem solving in a social context is misleading. Most "social problems" are conditions of society which are perceived quite differently by different individuals. (One man's tax incentive is another's loophole.) Decisions of government and private individuals or institutions may change the characteristics of the condition of concern and the degree of concern may intensify ("the problem has gotten worse") or diminish ("the problem has been solved") without overt intervention or without the generation of knowledge purported to "solve" the problem .

Therefore, it is perhaps better to speak of problem-oriented or problem-relevant research as an activity in which the techniques and facts of science are marshalled to improve our understanding of a particular aspect or condition of society about which there is substantial concern, with the implicit or explicit hope that by so doing the actors concerned with that area will function more effectively, the concern will be reduced, and the society will be perceived as, in some sense, improved.

Most of the concerns about society rest ultimately on a sense of equity (such as, some good of society is unfairly distributed among different classes of people-for example, health care) or on a sense of need for the whole society (some need is present and a good is required for society as a whole-for example, energy conservation. Consequently, the presumption that there is a problem to be solved (or a condition to be understood) is based ultimately upon moral propositions. Since views about such propositions can vary widely it is seldom possible to claim that a problem of society has been solved. People working in many types of institutions can contribute relevant insights which help society to form improved judgments and, although the contributions of scientists should not be minimized, scientists can provide only a part of the input to the overall process.

On "Science Institutions" Engaged in "Solving Problems of Society"

To examine the roles of science institutions it is useful to identify those areas or approaches where science characteristically contributes to the understanding of a condition of society. These are:

> Systematic analysis of existing information (such as, on energy prices);
>
> Systematic gathering of new information (e.g., on potential new energy sources);
>
> Formation of new relationships between new or existing information (e.g., a revised econometric energy model);
>
> Formation of new concepts or theoretical constructs (e.g., application of input-output analysis to study "net energy");
>
> Prediction of the likely consequences of particular actions (e.g., consequences of a gas tax).

All types of science institutions can contribute to such tasks. The common thread is that of intellectual activity in which people marshall and select relevant factual information to establish the validity of propositions about the particular condition of society being examined.

The principal difference between such investigations and traditional scientific research is that here the particular condition of society dominates the analysis and determines which data, techniques, and theoretical propositions are relevant to the analysis. In traditional research the properties of the natural world prescribe the arena and the methodology. Problem-oriented research is thus inherently interdisciplinary, requiring the blending of insight, data, and technique from all those fields of inquiry that can contribute to illuminating the condition being studied, including the "real world" of society and its institutions.

Each of the institutions in which knowledge is generated and used has special advantages and capabilities that can be discussed generally. Each also has characteristic disadvantages that inhibit effective interdisciplinary problem-related research. People within these institutions interact with each other in the course of their work -- the linkages are, more properly, between people rather than institutions, as the Workshop E group discussion found.

Universities have a central concern with the development of fundamental knowledge for its own sake and with the inculcation of the spirit and techniques of rational inquiry. Their great strength arises through high professional standards, integrity, and presence of young, inquiring minds. They are the natural home for basic research. Their strong orientation toward academic disciplines tends to make interdisciplinary, problem-oriented activities hard to organize and manage.

Contract research institutes, particularly those that are university-related, have the advantage of a format that allows assembling teams of individual scientists to work on a particular problem. The level of professional expertise in these institutes may be often somewhat lower than that at universities, but they contain many highly skilled individuals who are accustomed to work in a problem-oriented regime.

Federal R&D laboratories and other federal centers are usually dedicated to one class of problem (e.g., nuclear energy) and range from basic to applied and developmental interests with highly capable permanent staff.

Industrial firms and other for-profit R&D organizations typically assemble teams of researchers to work in areas related to the business interests of the company. They provide an appropriate locus for applied and developmental work leading to products, goods, and services either for company purposes or to fill a perceived need of a customer (such as the government). They are generally not appropriately oriented for public policy research.

Professional societies generally do not perform research or policy analysis, but can act as an organizer of the capabilities of their membership, particularly in focusing on policy questions that have extensive technical content, or in stimulating interchange and communication.

Public interest groups have a growing research capability in carrying out policy analyses and assessments of particular policy options. They provide one important link to the citizenry, but often exhibit a "point of view" which may represent a prejudgment of where the public interest lies.

State and local government organizations are typically weak in scientific expertise, but often has the most pressing need for substantial increases in data and analysis relevant to a public problem. Operating elements may have special data needs or capabilities.

The academies, but most particularly the NAS/NAE/NRC/IOM complex, do undertake research and study responsive to government needs. At least in the NAS case, substantial policy-relevant research is sought and directed from within the institution. The work of the NAS, while highly creditable and almost always of the highest quality, seldom qualifies as policy research. The scope of most studies is confined to determining the fact and uncertainty of only scientific and technical questions.

Linkages between such institutions customarily (and properly) arise through their common interest in a particular problem. Funding agencies in applied research, particularly NSF, often make linkages more explicit by funding consortia of different types of organizations to work on a common problem. In principle, each partner can then bring the special strengths of that institution to bear. University-industry and university not-for-profit combinations have been particularly successful. Linkages can also be stimulated by funding specific activities that bring researchers together with the potential users of the research results. Extensive, early, and frequent involvement of users (defined as those who may be affected by the research as well as those who may make contingent decisions) has been found by NSF/RANN to be one of the key elements in successful problem-oriented research.

* * *

WORKING NOTE 2: CHALLENGING THE UNSTATED ASSUMPTIONS AND PARADIGMS UNDERLYING THE (ORIGINAL) WORKSHOP STRUCTURE

(Daniel Alpert)

The questions as posed contain misleading assumptions that will send us off into unproductive directions. To clarify my point of view, I will quote the questions verbatim and then indicate my concerns.

Why are linkages needed between distinct science institutions? What are some examples of existing linkage patterns? (Original workshop question.)

This is the wrong question.

Science institutions or science-related institutions include R and D centers in universities and private research institutes, foundations, government agencies, professional societies, and public interest groups. They have separate or distinct roles, such as basic and applied research, teaching and other training activities, providing support for such activities, or questioning the quality of research products. Through interactions between these separate organizations, it is assumed that these roles complement and intersect one another. For example, the research center, funded by a private foundation, produces information that can be incorporated into a curriculum designed to train future managers or decision-makers. It is generally assumed, although sometimes strongly questioned, that universities consistently perform better in the areas of education and basic research, and that research institutes are better suited to produce policy research.

In the technological science-engineering area, there are some generally shared paradigms involving relationships between basic research, development, engineering, manufacturing design, production, marketing, etc. Typically, there is a different organizational structure for each phase of the process from research to the marketplace. The different kinds of R&D institutions within a given corporation have developed workable relationships and, generally speaking, mutual respect. Ideas are transferred from the research lab to the development lab, typically by transferring people from the research laboratory to the development laboratory. The technological pipeline is people!

In the field of societal problems, our basic research is largely isolated from the operations of governmental units. We do not have linkages, partly because we do not have the organizational structures to support such linkages. We have policy science and technology assessment; these are all right but they're inadequate to handle the vast array of problem situations.

The linkages between "distinct science institutions" are excellent. The communications among physicists (or among chemists, or among psychologists) at universities, national laboratories, industrial laboratories, and government laboratories is remarkably good. As physicists or chemists they share a common culture that crosses institutional boundaries and have little trouble in the development of linkages. To foster collaborative activities on societal problems, we should strengthen the linkages among the various disciplinary or professional groups within the same scientific institution. This problem is greatest at universities and probably least at industrial laboratories, where research and development is typically organized around problem-oriented projects rather than disciplinary departments.

The critical problem is the relatively small linkage between any of the science institutions, including most government laboratories, and the operating units of government at any level. (The Department of Defense might be viewed as an exception to the above statement in view of the strong interactions of the DOD with scientists and engineers at various kinds of institutions, including universities.) Unfortunately, the problem of defense should go beyond the preparation for war; there are few linkages between the scientific community and agencies dedicated to peace, such as the State Department, and the Arms Control and Disarmament Agency.

By contrast with the public sector, the private sector has evolved a relatively strong symbiotic relationship between industrial R&D laboratories and the corporate management of major companies. Scientists and engineers in industry are regularly promoted (not transferred or temporarily assigned, but promoted) into positions of responsibility in the corporation. In view of this linkage, deeply imbedded in the private corporation, managers and research scientists have learned to speak each others' jargon; managers are far more likely to comprehend the metaphors and paradigms that are used by scientists and vice versa. The key linkage that is missing in dealing with societal problems is the lack of a symbiotic relationship between the R&D community as a whole and the operating divisions of government. This represents a rather profound problem; it is not likely to be understood without examining the basic assumptions of science and its relationships to society.

Not only is the active scientific research community divorced from government activities, but many of the agencies that sponsor research such as NSF, DOT, and HUD are either

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disconnected from the operating government agency, disconnected from the R&D community, or both. Gerald Edelman has described this problem as follows:

> The modern heresy is not mechanism and reductionism -- it is the encouragement and continued development of two nonintersecting disciplines, one concerned with power over nature, the other with power over men. The heresy is to assume that from either side, these disciplines are intrinsically at odds with each other or inherently evil.

These nonintersecting disciplines are the scientific community and the legal-political community that runs our public institutions.

What linkages are required in order to transfer research information to the social problem-solving process? How can this transfer be made more effective? (Original workshop question.)

One of the principle conclusions from our workshop was that the transfer of information is not a key problem. Information is currently being generated and transferred in huge volumes; there is an overload of information; reports are collecting dust in virtually every government agency in the country. (I call attention to our statements: "Knowledge does not introduce change; people introduce change using all kinds of knowledge." We also pointed out that people in power often isolate themselves from "the facts," sometimes because they can actually perform better without them.)

The above observations again challenge the questions around which the workshop was initially structured. These questions contain within them a number of unstated assumptions and paradigms. These paradigms are probably consistent with the conventional wisdom; my concern is that the conventional wisdom is obviously not working.

What will prompt traditional institutions to engage in networking? How can individuals engaged in these catalytic efforts be rewarded and maintained within their academic environment? (Original workshop question.)

Again the above question contains implicit assumptions. If we could answer these questions we would not be faced with the problem of increasing effective contributions to solving public problems. The conventional reward system of our academic institutions does not provide incentives to work on problems posed by society. The dilemma is deep and subtle; for example, the administration of most of these institutions would like very much to support "public service," but the reward system is dominated by a concern for research and scholarship.

How Can We Rephrase the Questions?

It is difficult, and perhaps not particularly useful, to reformulate the report of Workshop E in a revised questions format. The report is a valid statement of where we were. To be sure it does not answer the tough dilemmas; but it tries to state some of them, however vaguely. Perhaps rephrasing the three questions as follows highlights what I believe are the more productive questions to ask:

- What kinds of new or strengthened linkages are needed between science institutions and the operational agencies of government?
- What linkages are required in order to enhance the social learning process? How can the academic learning that goes on in universities relate to social learning that is the responsibility of government?
- What will prompt traditional institutions to support new explorations in problem-solving? How can the individuals engaged in these catalytic efforts relate to individuals in other institutions and in government agencies?

Critical Questions

The critical questions that came out of the conference are:

- What new paradigms (roles, research activities, etc.) are needed for applying R&D to problems in the public sector?
- What new organizational structures or networks are needed to develop symbiotic relationships between decision-makers in government and members of the R&D community?
- Can the AAAS (or any other organization) hope to develop mechanisms by which scientists can talk to nonscientists in a meaningful way?

In his final remarks, West Churchman suggests a need for more interactions between scientists and persons in other societal roles including lawyers and politicians. This is an important possibility which AAAS should consider. One might look for a better way of overcoming the traditional disdain of universities for entrepreneurial activities which would encourage networking. Professors don't disdain the art of entrepreneurship nor does the university environment particularly discourage it. Professors are very good entrepreneurs. Unfortunately, they often propose to solve problems they don't understand, or worse yet, tell politicians that basic research will some day solve them. We have not explored whether or how science can be introduced into the process of running our government. What is more we have not explored how esoteric knowledge relates to exoteric knowledge, as Churchman said in his keynote remarks. This represents a critical area for further deliberation.

" ... federal agencies are major contributors to the difficulties associated with doing interdisciplinary problem-oriented research,... there is a profession of need for interdisciplinary research, but an unwillingness to run the risks that are inherently involved."

Don Kash

F.

WHAT IS THE "DEMAND" FOR INTERPROFESSIONAL PROBLEM-ORIENTED WORK?*

Richard Bolt
Clark Abt

Five questions were offered to this workshop group as a guide for their discussion. This summary is organized around those questions since, in fact, the discussion followed them quite closely.

DISCUSSION

1. In Developing Problem-Oriented Research, What Kinds Of Users Should Be Identified? Are There Difficulties In Identifying Specific Markets?

- The principal users are executive agency and legislative policy-makers, at the federal, state, and local levels, who are concerned with solving social problems. In particular, policy-makers who are concerned with social problems which require the application or creation of knowledge from all of the sciences and engineering. What the interprofessional problem-oriented and policy research does is to supply an additional analytic capacity to policy-making; it tends to replace executive guesses with empirical data and explored alternatives.

  The policy-makers themselves will not generate a demand for this capacity, and they will not explicitly ask for policy-oriented research. The agency research program managers must perceive this need. They must foster a demand for "translating function" mechanisms to channel policy-relevant research findings into the decision-making and policy-option process.

  The Agricultural Extension Service was one example of such a translating funtion. It functioned very well between individuals, but less well when it attempted to serve groups.

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* Richard Bolt (Chair), Clark Abt (Reporter), Howard Davis, Burton Dean, Thomas Glennan, Kirsten Gronbjerg, Daniel Horvitz, Ernest Powers, Edward Poziomek, Susan Salasin, and F. Tomlinson Sparrow.

Unlike military and space research, where the government is both the sponsor and purchaser of the research product, the market for civilian problem-oriented research is highly dispersed and fragmented. If the researcher is not responding to a specific demand, some attention must be paid to the "generalizability" of research results. In some cases, this consideration becomes the overriding criteria for federal sponsorship of research projects.

Another criteria is the implementability or actual short-term utility of technology, particularly at the local level. Whether it is policy analysis or innovative technological systems, a transfer process must take place between those who generate the research and those who apply it. In general, "technology push" is not as effective as policy or social need "pull" in getting the results of science into the public market.

2. What Are Some Examples Of Existing Legislation Which Require Interdisciplinary Research? How Has This Research Been Developed?

- The approximately 1% of allocatable funds required for program elevation in the Elementary and Secondary Education Act (ESEA), Title I (Aid for Disadvantaged Children) is one notable example. This research is incompletely developed. Other examples of existing legislation which require interdisciplinary research include: the major proposals for welfare reform, malpractice legislation, national health insurance, and environmental control legislation. All legislation that involves the social and economic impacts of economic transfers in cash or in kind, and major physical changes in the environment are intrinsically interdisciplinary problems.

3. What Are Some Views Of Private Industry Or Research Firms With Regard To The Market For Problem-Oriented Research? Is This Perceived As A Growing Area Of Activity? How Is This Trend Related To Contract Dollars And Personnel Figures?

- It is not perceived as a great market, because of the lack of continuity in funding. The constant exit of government clients increases the lead time and front-end costs of research contracts. Projects need to be refunded every year. Contracts are not directly tied to past performance, and it is very difficult to establish product or service loyalty.

On the other hand, the market is very honest and is unusually free of corruption. On the whole, the allocation of funds is based on merit more than in other businesses. This may be a result of early congressional criticism. It should be noted that there is a substantial cost built in to ensure objectivity of the research award and the effective use of the researcher. We have brought to bear on the research market the canons of public administration, not the canons of good research management, maybe because there are none. It's not a particularly effective market, but it's quite fair.

In order to determine whether this market is perceived as an area of growing activity, it is necessary to examine several indicators:

(a) Private investors are not especially willing to invest in social R and D: very little private capital seems to be going into this area. (b) However, it is an area of growing activity, and there appears to be a high return on investment -- although that return may not be achieved by the original investor but rather the society at large. People are willing to invest in the capacity of a research group to do this kind of research, if not a specific product. (c) Investors don't understand this market -- they cannot get used to the concept that you are selling your capacity to learn about something rather than a particular technical skill.

The general implication from these indicators is that there won't be much private investment capital going into problem-oriented research in the near future.

Research firms that have already invested a substantial amount of private capital into problem-oriented research are Abt Associates, Arthur D. Little, Battelle Memorial Institute, Mathematica, Planning Research Corporation, Rand Corporation, Research Triangle Institute, Stanford Research Institute, Systems Development Corporation, to mention only a few.

As perceived by some federal sponsors of this kind of research, this market is growing very fast. It is suggested that the $200 billion investment in social services, which began under the Johnson Administration, has spun-off considerable investment in R and D and is growing at 10 to 20% per year. This research is carried under different purposes at different times -- sometimes it is used for problem-solving, sometimes it is used to evaluate cost effectiveness of the service programs. However, recent cuts in social research and policy-oriented research budgets suggest that this growth is experiencing a strong setback in some areas, and is not as large a market as might be expected. In the workshop there was substantial disagreement around this issue.

Such cuts in mission-oriented research budgets seem to give rise to a process encouraging the decentralization of such research. For example, studies become funded at the state and local government levels. This decentralization is viewed by some as a political maneuver, generating support for the agency's research program at the local level which is then brought to bear on the budget critics. Some critics of the research resulting from this decentralization claim that it results in "shoddy" products of little value. An important policy question may focus on this process: How to effectively decentralize a research program and maintain quality of research product?

This shift of emphasis to state and local government research also creates a new concern: How to create a sophisticated capacity which will generate a demand for federally sponsored R and D: One example was cited: the establishment of a network of technology transfer agents in the city governments. This project, coordinated by Public Technology Inc. with support from the National Science Foundation, is aimed at building an awareness among the city managers of the value of technical data bases and creating a "demand pull" for such information. One substantial risk involved in funding and operating such a program is that many local governments see this "building capacity" money as merely a supplement to their operating program budgets.

4. Are The Long-Range Plans Of Research Institutes Responding To The Market For Problem-Oriented Research? Do These Plans Foresee A Need For Organizational Change As A Result Or Pre-Condition For Carrying Out Effective Interprofessional Research? What Are Some Examples Of These Changes?

- Universities are continuing to set up institutes to do problem-oriented research, but very few of these manage to survive more than five or six years. Most of these centers as presently structured can not provide career opportunities for professors in the university environment. There have been some success stories, but it is not clear just what the key factors for this success are.

These research centers have also placed stronger emphasis than other disciplinary-oriented research groups on the management of research. This emphasis on the management of research is important. Part of the management strategy for these centers has been to bring in such skills as evaluation research, computer simulation, operations research, systems analysis, etc. These skills are unifying tools and provide a means for bringing researchers from different disciplines together.

5. Do Certain Kinds Of Problem-Oriented Research Create A More Attractive Market Than Others? What Are The "Attractive" Features?

- Yes, certain kinds of research are more attractive. The features which count as a plus are creativity in research design, continuity of funding, and promise of application of results. These features need to exist from the very beginning of the research process, usually reflected in the research contract. Detailed specifications, however, tend to reduce the opportunity for creativity. Institutional stability of the proposed problem-oriented research and federal sponsors who are both supportive of that stability and consistent in their view on the role of the resultant product are also critically important attributes.

> "The critical problem is the relatively small linkage between *any* of the science institutions, including most government laboratories, and the operating units of governments at any level."
>
> *Daniel Alpert*

IV.

CONFERENCE PAPERS:

UNDERSTANDING THE PROCESSES

" ... We can't find any of those methodologies that work and about all one can do in interdisciplinary studies aimed at informing public policy is organize groups of people from different disciplines or project groups or task force groups. The organizing mode really is the problem definition."

Don Kash

OBSERVATIONS ON INTERDISCIPLINARY STUDIES AND GOVERNMENT ROLES

Don E. Kash
Director, Science and Public Policy Program
University of Oklahoma

This paper has two major parts. The first part represents a summary of the development of a study of offshore oil and gas carried out by the Science and Public Policy Program at the University of Oklahoma. The second part represents a summary of the institutional levers necessary if a university is to have much chance of carrying out interdisciplinary problem-oriented research. The conclusions presented in the second part of the paper are drawn from experience we have gained in carrying out five studies. All of these have been concerned with energy and all have been carried out by interdisciplinary teams.

The Science and Public Policy Program was established to do technology assessment, that is, research which has as its purpose informing public policy about the unanticipated consequences of developing and using technologies. The underlying assumption of such research is that technology is one of the major causes of change in American society.

PART I

POLICY RESEARCH: A SPECIFIC CASE

The research that is central to the first part of this paper was carried out by a team consisting of three engineers, a physicist, a biologist, a lawyer, and two political scientists. One of the central reasons for selecting outer continental shelf (OCS) oil and gas development for study was its expected policy relevance. We purposely chose a topic that we hoped would be of importance two years down the road when the research was completed.

This part of the paper includes two components. Initially, it sketches a general picture of policy-making systems as we have perceived them in our work. Secondly, it summarizes our experience with policy-oriented research.

Policy-Making

My colleague, Jack White, has summarized a general picture of the normal policy-making process and elements which can disrupt that process as follows:

> Most students of our political system generally agree that in the United States public policy-making and administration in any substantive policy area usually involve only a few actors or participants on a continuing basis. Continuing, direct participation is usually limited to those actors who have official responsibility plus those who have an unambiguous, obvious, and direct stake in what public policy in a particular policy area is and how it is administered. Typically, these actors are highly organized and bureaucratized to promote their particular interests, as are the American Medical Association and the American Petroleum Institute, for example.
>
> However, our political past also demonstrates that a variety of stimuli or factors can upset "policy-making as usual" in any area of public policy. The attention of additional participants may be attracted by:
>
> 1. A catastrophic event such as an oil spill in the Santa Barbara Channel;
>
> 2. A general change in values such as that which appears to have occurred recently toward environmental quality;
>
> 3. A shortage in the availability of a needed commodity such as petroleum products made visible by shutting down assembly lines, closing public schools, and limiting gas sales; or
>
> 4. An individual such as Rachel Carson or Ralph Nader.1/

Our research experience has been in a policy area where "policy-making as usual" was disrupted and new participants attracted in by a combination of the first three stimuli.

POLICY SYSTEM

Given our explicit goal of providing policy advice, we had two initial needs: first, to identify both existing and potential policy problems; and second, to identify existing and potential participants in the outer continental shelf oil and gas policy system.

Our approach to the study of this system was heavily influenced by the work of Don Price.2/ He has been an articulator, not only of the view that most policy is organized around substantive activities, but also of the view that most policy options are defined by those people and/or interests who have a continuing involvement in the substantive activities. Participation in policy-making requires either knowledge of or a stake in the outcome of the substantive activity. The possibility of playing a role as a policy analyst and advisor depends on having knowledge of the substance of the activity for which policy is being made, and having policy-makers aware that you have that knowledge.

Policy-making involves, in the abstract, a three-step process: first, the identification of possible substantive actions; second, the identification of policy options which involves an assessment of who will enjoy the benefits or suffer the costs associated with particular substantive actions; and third, the selection of particular policy options. My colleagues and I have conceived of each policy system as having two components. One is the policy sector and the other is the policy community.

Policy Sector

The policy sector is defined and limited by the set of substantive actions potentially available. Our view is that substantive actions are heavily determined by the limits of technology. The boundaries of the OCS oil and gas policy system, then, are defined by the state of the art of that technology. For instance, natural gas produced in waters beyond the depth where pipelines can presently be laid is not a policy option that can be used for meeting our gas shortage.

The outer boundaries of a policy sector, then, are set by substantive or tangible considerations, not political ones. A policy sector can be viewed as consisting of three categories of substantive actions. I find it useful to think of a policy sector as consisting of three concentric circles, each of which represents a category of substantive activities defined by technology.

The outer circle consists of that set of substantive activities that have been proposed, but which are universally recognized as being beyond the state of the art. For instance, we cannot presently build pipelaying submarines which would allow us to overcome water depth limitations, but such technologies have been proposed.

The middle circle includes those substantive activities on which there is disagreement. There is, for instance, disagreement among professionals on the capacity to build production platforms in 1,200 feet of water. Present experience has only taken us into four to five hundred feet of water.

The center circle of the policy sector includes those substantive activities which everyone agrees are available. For instance, we have a demonstrated capability to lay pipelines in waters up to 400-500 feet.

Policy Community

The policy community consists of those actors who either define the boundaries of the policy sector, or those who have a stake in the outcome of policy choices. Stated differently, participation in the policy community depends on expertise with regard to the substantive activities and/or vested interest in the outcome of policy choices. When policy communities are stable, most members have both expertise and a vested interest. When instability is triggered, new participants enter who have little substantive knowledge of the options.

It is expertise, however, that continues to define the boundaries of the policy sector and locates the policy options within the three circles. Policy research is usually generated because it is perceived as having a potential for contributing to the definition and assessment of possible substantive actions. It is with regard to the identification of substantive actions that knowledge and information are viewed as being important.

On the other hand, the right to participate as a decision-maker in choosing which of the substantive actions will become policy rests much more heavily on actors having a vested interest in the outcome. Two kinds of vested interests appear to exist. One kind is associated with tangible costs and benefits such as how much profit a company will make from developing OCS oil and gas. The other is associated with institutional vested interest concerning who has the right to make the decisions. For instance, does the industry or the Department of Interior have the right to specify which technology will be used or how fast oil can be produced from a well.

When one sets the policy community in the policy sector, the actors distribute themselves among the circles depending upon whether they are there because of vested interest or expertise. In general, there are minimal policy options in the outside circle where the substantive actions are clearly beyond the state of the art. The only choices are those associated with taking research and development actions aimed at bringing blue sky proposals within the substantive state of the art. Present interest in fusion energy would fall in this category. Actors with traditional vested interests in costs and benefits aren't much interested in outer circle actions.

The reverse is the case with the center circle policy choices where all the substantive actions are within the state of the art. The issues here are over who enjoys the benefits or suffers the costs and actors with only substantive expertise play a minor role.

It is in the middle circle of the policy sector that both vested interest and expertise play a minor role. In a rapidly changing technological society most real policy options exist in the middle circle. These options are by definition ones that involve interdependent political and substantive judgments and they generate most of the demand for the use of policy research. The policy researcher is most frequently used by those who have a major stake in policy outcomes but limited substantive knowledge. The most useful policy research appears to result when it reflects understanding of both the substantive and the political issues but reflects no obvious vested interest. Such research acts as a filter to determine the practical choices. This research aims at finding a set of middle range possibilities between those who would prohibit all offshore development and those who would have unlimited or unregulated development. Next most useful is research which communicates understanding of the substantive issues. There is little work for researchers who understand only the political issues but who have no stake in the outcome.

Let me note that the scheme I have just sketched is an after-the-fact creation, but it represents in general terms the model of the policy system we perceived and the role of the policy researcher in it. I repeat, we started out with the objective of providing policy advice. One of the reasons for having the interdisciplinary research team was to allow rapid definition of both the policy sector -- the three circles or categories of substantive actions -- and the policy community -- those who participate either because of expertise or a stake in the policy outcomes. Interdisciplinary research is basic to the identification of policy options in a rapidly changing area of technology.

OCS SYSTEMS

Our first task in the OCS study was to describe the two components of the policy system. For the offshore oil and gas sector, we identified the three categories of substantive actions, that is, available, uncertain, and potential technological actions. Starting with those categories of technologies, we next identified the policy community that linked to these technologies.

In general, the OCS oil and gas policy community had at the time of this study the following actors:

(1) In the Congress: the Senate and House Interior Committees, the Senate Commerce Committee, the House Science and Astronautics Committee, the Congressional Research Service, and the Office of Technology Assessment;

(2) In the Executive Branch: the Department of the Interior, the Council on Environmental Quality, the Environmental Protection Agency, and the Department of Commerce;

(3) In the petroleum industry: the National Petroleum Council, the American Petroleum Institute, and the individual companies;

(4) In the environmental interest group category: the Natural Resources Defense Council, the Sierra Club, and the Environmental Defense Fund.

Continuous Contact with the Policy Community

To assure that our findings would be given consideration in this policy community the minimum requirement was for us to communicate that we had substantive knowledge and no vested interest in the outcome. We did this by maintaining continuous contact with all of the above components of the policy community. I would note here that our lack of a stake in policy outcomes was regularly questioned. Being from Oklahoma, the environmentalists were generally suspicious that we were puppets of the industry. Being from a university, the industry generally suspected that we had an anti-industry-environmentalist bias.

Our interaction with the policy community during the study had five components:

(1) We appointed an oversight committee with members representing each of the four major elements of the policy community plus two professors. Individual members of this group repeatedly reviewed the papers we wrote. Additionally, we had the group together for three two-day review sessions covering overall drafts of the study at an

early, middle, and final stage of the study.

(2) Secondly, we made extensive use of consultants from every sector of the policy community.

(3) Third, we used repeated interviews with people from the whole range of the OCS oil and gas policy community.

(4) Fourth, we sent out papers to anyone we could get to read them.

(5) Fifth, we held a one-week conference at a boys' school in the middle of Maine. About 80 people attended from all points in the policy community.

The above activities had two purposes. One was to get all the help we could in identifying policy options by gaining a thorough understanding both of the policy sector and of the policy community. The other was to alert and inform potential users of our study. Members of this policy community find few things more distasteful than advice by ambush. Stated differently, policy communities work by evolving consensus and they like nothing less than surprises.

Our pattern of continuous contact had a major payoff we had not foreseen. To appreciate this point, I should note that no one on our research team knew anything about offshore oil operations in advance of the study. Some of our initial papers reflected a level of ignorance and shoddy quality that was embarrassing in the extreme. Most of us on the team were certain that airing such papers would ruin our reputations forever. As it turned out, showing our early work, warts and all, was a major factor in establishing our credibility. At one time or another we misunderstood nearly everything and were on every side of every issue.

The effect of this evolution was to demonstrate that we did not start out with a predetermined position and build a case to support it. Further, the quality, the understanding, and the hard data improved with each draft, and we began to build a reputation for having the capability to learn.

The final draft of the study was submitted to twenty-five people for review. We picked ten reviewers and the National Science Foundation, which funded the study, selected fifteen. Reviewers ranged from people with clear vested interests to academic experts in specialized areas. The reviews were distinctly favorable. Of the group, only one was overwhelmingly negative and it was by a political scientist.

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This favorable set of reviews set in motion a kind of cumulative pattern of noise throughout the policy community. The rumor system was now building this up. One experienced bureaucrat told me, at this point, "You've got it made. It is now a part of the conventional wisdom of people involved in this area that yours is an important well-done policy study. Even those people who don't like it will not be able to write it off." He went on to note that most people will never read it in total, rather they will only use those elements they have an interest in.

Review and Distribution of Results

All of the preceding might have happened and the study would still have been filed and forgotten had it not been for two other factors. Most important was the fact that two days after the study was delivered to NSF on April 14, the President went on television and proposed the resource leasing on the OCS would be tripled. At the same time, he directed a one-year study of OCS oil and gas operations by the Council on Environmental Quality (CEQ).

Another major factor was that a strategically placed civil servant had read the study and decided it warranted major attention as a starting point for the presidentially directed study which was to be carried out by CEQ. The first step in that study was a series of public hearings at six different locations in the U.S. The announcement of the hearings designated our study as one of three background documents.

In preparation for those hearings we were requested to prepare a one-day briefing to be held at the auditorium of the National Academy of Sciences. NSF agreed to purchase 1,500 copies of the book which resulted from the study for distribution to interested parties.

In the study, we had made 39 specific recommendations and these were the basis around which the Washington briefing was organized. Part of the briefing format involved critiques by representatives of government, industry, and environmental interest groups who had no previous advisory role in the study. Fifteen hundred people were sent letters of invitation. Some two hundred showed up.

Every component of the briefing made use of slides and we made every effort to make the briefing concise. A terribly difficult task for academics,I might note.

In conjunction with the briefing, we held a press conference which attracted the major wire services as well as several major publications.

Following the briefing, NSF provided us with support which allowed us to offer advice to anyone who was interested in the work or in using us as advisors. We followed a policy of offering our assistance to anyone who was interested at no cost and on short notice.

Given the development of the Arab oil boycott and the associated emphasis on OCS oil we were nearly overwhelmed with requests. Part of the explanation for our role as policy advisers goes back to an early point in this paper. We were perceived as being knowledgeable about OCS oil and gas operations, but more importantly we were seen as the only group with that kind of knowledge who did not have a stake in what happened. The central point is that there was almost no substantive expertise outside the industry and the Department of Interior.

The other element that made us attractive was that our recommendations were specific. That is, the proposed discrete actions that could be implemented. Both the executive agencies and the congressional committees found specific policy options that could be adopted immediately, that is, the recommendations provided courses of action that could be used to respond to the growing although very unspecific pressures for some kind of action. Portions of the recommendations appeared in six Senate bills and we were asked to comment on various other legislatively proposed options.

Our policy advisory role has been primarily in the portion of the policy sector covered by the middle circle where the substantive actions are uncertain. We tried to focus our advice on the substantive actions but such advice always influences the value choices. We found ourselves continuously resisting pressures to adopt positions resulting primarily from strong value biases.

The general value positions in this policy system pose environmental concerns against energy concerns. Energy needs to pressure the government rapidly to develop oil in waters that are much deeper than any in which we have present experience. Environmental needs call for minimizing potentially damaging accidents and therefore, call for a go-slow attitude on deep water operations. A conclusion by our group in either direction would have been rapidly used by one of the contending interests, but we did not feel the evidence or our understanding allowed us to come down on either side of the issue. It is extremely difficult to resist taking a position on this kind of issue.

By this, I do not intend to imply we refuse to take positions, quite to the contrary, taking specific positions is the essence of policy advice. Rather, what we feel is necessary is to protect our reputation for independence by taking positions only after we have an understanding of both the substance and the values involved.

Five Elements in Policy Analysis

The preceding description identifies five elements which were important in this effort at policy analysis and advice.

First, the opportunity to act as a policy adviser rests on substantive knowledge. I believe this is a point which social scientists should ponder at some length. For me, it says that social scientists in universities are unlikely to play a major role as policy advisers unless they do more of their work in an interdisciplinary context. Although it may be possible for a single person to learn the substance of a policy sector, that is a most inefficient approach. The need is for institutional mechanisms in universities which make interdisicplinary research much easier.

Second, policy analysis that is perceived as being disinterested has the maximum opportunity to inform the diverse members of a policy community. This is a difficult point to convey and it requires continuous contact with potential users from the start of the research. It is hard for those of us in the university to subject our early work to unsympathetic reviewers. Yet such a review process is central to conveying the lack of bias.

Third, timing is critical in determining the impact of policy analysis on policy-making. The lesson John Gaus tried to teach many years ago about the ecology of government is appropriate here.3/ Public policy is driven by a complex interaction between the human and natural world. It is the result of a complex network I do not understand.

I am convinced, however, that the impact of policy analysis on policy-making is heavily dependent on the movement of the ecology of the policy system. Chance is particularly important in determining timing. We did our best to select a well-timed focus for our study. The key in insuring its use, however, was an Arab oil boycott. There was no way of knowing that in advance.

Fourth, the policy adviser's role in policy-making is directly correlated to his ready availability to the policy community. Being ready to respond on call is painful, but policy decisions do not appear to wait on the availability of the adviser. We repeatedly found ourselves on overnight call.

Fifth, specific proposals for action enhance the probability that policy analysis will be used. Lindblom's description of policy-making seems to correctly describe the decision-making process.4/ Most decisions are made in response to the heat of the moment and advice which proposes discrete and specific options that are responsive to both tangible and political considerations is desired. Generalizations, which must be translated into specifics by decision-makers, appear to get lost. Similarly, although I know it runs counter to conventional wisdom, we have found specific recommendations better than cost-benefiting options. In practice if the cost-benefiting is well done it almost always turns out to involve recommendations anyway.

PART II

REQUIREMENTS FOR POLICY RESEARCH IN THE UNIVERSITY

The view that interdisciplinary research teams are needed to adapt science to social problems reflects a growing belief that in a technological society the social and physical systems are inseparable. Although at one level the belief in an interdependent social-physical system is now part of the conventional wisdom; at another level our thinking, many of our organizations, and most of our knowledge reflects a quite different wisdom. That is, a reductionist wisdom. The university represents reductionism in its finest organizational form. University organization has responded to very compelling conditions. They are that we have only been able to understand by dividing the world into parts.

The reductionist approach which dominates the university also has been the norm in understanding and in managing U.S. society. In practice, U.S. society rests on a Constitution which requires managing by dividing power. For instance, our society has found it useful to separate church and state, government and business, and so on. Even at specific policy levels division has been our pattern. We have, for example, managed energy on a fuel-by-fuel basis, viewing each fuel as if it were a self-contained system. In nearly every area of society this pattern of management appears to work less and less well. Because of the recent perception of scarcity in the energy area, there is an immediate need to understand and manage all energy resources together. The problems of developing energy policy as opposed to oil policy are difficult.

Our paradigms for guiding such synthetic management are very limited. There is no theory from which we can deduce what is needed.

Substituting Organization for Methodology and Theory

The way in which we've organized our Science and Public Policy Program is a reflection of the fact that there is no theoretical basis upon which one can build or develop this kind of interdisciplinary research. It would be a lot easier if there were some paradigm which worked, but the problem is, from our point of view, even more difficult. There's not a paradigm. There's not a theoretical base, and there's not really an appropriate methodology.

There are a lot of methodologies around and people who try to do this kind of research, including ourselves, regularly try to find some methodology which will give some meaning to the work that's being done. In fact, however, we've just not been very successful in finding methodologies that work.

Now, I want to offer some comments on that because it really is central to our own experience and certainly my own impressions. There is in the university, but I think in all of the other organizations that are involved in trying to do this kind of work, a compelling belief that if we just work hard enough at it we can ultimately understand the interactions between science and technology. Most people feel that we can ultimately develop a theory that will allow us to understand those interactions, and in the absence of that theory there is this tendency to try to substitute methodologies, one or another form of cost benefits analysis, use of delphi simulation, modelling and so on. If you do the work as we try self-consciously to, that is do it with the aim of influencing or informing or having some impact on public policy, these methodologies don't work out very well.

Now, that pressure to be like a discipline in the university is very tough to resist. It is reinforced by the fact that most of the federal agencies are populated by people who share the same kind of tendency. The pressure at the present time, I think, is very clearly in the direction of proposing to study science-society interactions, using again one or another of these methodologies.

The view from the federal agencies position is two-fold. One is this tendency to want the structure and the order and the understanding that comes from a paradigm or a methodology. But, there's another and more doubtful pressure and that is that federal agencies find interdisciplinary studies to be a lot safer if they start with a paradigm -- if the agency can know in advance that the study is not going to turn out anything that will be uncomfortable. And, I might say that part of the pressure for using methodologies in universities, and I think in other research organizations, is much the same.

Now, what we've done is to say we can't find any of those methodologies that work and about all one can do in interdisciplinary studies aimed at informing public policy is to organize groups of people from different disciplines or project groups or task force groups. The organizing mode really is the problem definition. And you can define it -- in our case we usually try to structure the investigation around some particular technology, trying to examine the impacts of that technology on society. But, the only thing that holds our study together usually is the hardware around which we are building the study. Subconsciously we've taken the view that what you have to do is substitute organization and procedures for what would be better done with an adequate methodology or body of theory.

We do have a limited reservoir of empirical experience in managing complex systems in such high technology sectors as defense and space. Although that experience is only marginally applicable, it is the best we have. Many argue that experience with systems management in high technology areas provides little guidance in the contemporary period because our present problems have a much larger social component relative to the technological component. Those who make this argument conclude that the organizational and management patterns used to successfully apply science to space and defense problems will not work in the new domestic problem areas.

Recognizing these differences many of us believe, nonetheless, that we must attempt to apply science to society's problems and that we have no choice but to attempt to evolve arrangements from known organizational and management approaches. We arrive at this position not because of the demonstrated success of these approaches in domestic problem areas, but because of a lack of alternatives. The ability to not only link people with diverse specialities together, but also to link science, government, and industry seems essential if we are to resolve our domestic problems.

The need is to build organizational arrangements which include these diverse linkages. But these new organizational arrangements must be self-consciously sensitive to what we know to be characteristic patterns of human behavior. Behavior that is nonrational, short-sighted, and self-interested. When viewed in its group or social form, human behavior is focused more on escaping a multitude of conflicting fears than it is on achieving positive goals. Such behavior is the essence of politics.

The University Role

Many people debate whether the university should have a role in research aimed at helping resolve short-term

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political issues. In practice, our constituencies will give us no choice--they will, and are, demanding that we play a role. Ideas and action are too closely linked in the technological society for anything else to happen.

I believe that doing research that has short- to mid-term use is both a responsibility of and a value to the university. To produce applied interdisciplinary research, however, requires that universities create new organizational arrangments. In this case, the need is to create organizations in advance of a supporting knowledge structure such as the traditional departments have.

Both the value system and present organizational structure of universities make this difficult. As those who have tried to develop them can describe in detail, natural rejection mechanisms are triggered in universities when such interdisciplinary problem-oriented organizations are created. Strong support from an administration, lots of money, and a strong leader may serve as an immunosuppressent, but a history of such organizational efforts would have to be entitled, "A History of Failure." The reasons for failure are well known, basic disciplinary research has the highest and provides the biggest rewards. This status position is regularly reinforced with the political argument that doing research on social problems will anger supporters of the university and decrease support for basic research. I have never seen anything other than anecdotal evidence to support this view and I'm not convinced. At least, as much anecdotal evidence exists in support of the opposite view.

The major systemic reason for university rejection of problem-solving research results from the fact that knowledge is organized into intellectual structures we call disciplines, and those structures provide the rationale for departments. Only discipline-based departments have a continuing capability to provide such organizational rewards as tenure and promotion. The rationale is that only the disciplines provide a dependable basis for judging the quality of individual performance. The assumption is that disciplines have what Kuhn calls paradigms which provide a standard of quality. By comparison, interdisciplinary work presumably has no obvious standards of quality. This results in interdisciplinary problem-oriented work attracting weak sisters and charlatans like sugar attracts ants because they hope to gain organizational rewards without having to meet difficult standards.

Faculty members presently get involved in interdisciplinary activities for one of three reasons. In the first and most exciting case they do so because they find that their discipline has become an intellectual straightjacket blocking their research and intellectual development. Numbers of people with an intellectual interest in the environment have found this to be so. A modified version of this case is the young researcher who is driven by some social commitment which cannot be satisfied within his discipline. The second case is the naive researcher who either doesn't understand the reward system of the university, or thinks he is good enough to beat it. He is a sad case because he regularly loses. The third and most frequent case, especially relevant at the present time, so my administrators tell me, is the researcher who will work on interdisciplinary problem-oriented research since he or she can't find a "regular" job.

From the point of view of the applied research organization in the university this is a shakey manpower base to build on. Assuming that high-quality people enhance the chances of applying science to society's problems, an organization with that purpose in the university should be able to hire such people.

Creating an Interdisciplinary Research Team

But, having good people does not insure the capability of producing high quality applied research in the absence of theory or methodology. That requires creating a productive research team out of the disciplinary apples and oranges. Creating and sustaining such a research team requires, at a bare minimum, the following institutional levers: (1) a flexible, hard budget, (2) the capacity to reward with tenure and academic rank, (3) appropriate physical facilities, and (4) continuing symbolic strokes from the university administrators -- that is, public statements by the president that he loves you and that you are doing great things. (Please note I have explicitly rejected the notion that interdisciplinary research can be derived from some methodology.)

The hard money budget is at least as necessary for symbolic reasons as for more tangible reasons -- that is, it projects the image of an organization that is going to have to be lived with on a long-term basis. By being flexible the budget gives the interdisciplinary research unit in the university an ability to provide long-term patronage to the departments. One has to be able to juggle hard and soft money on a continuing basis to survive in a fluid political environment.

The ability to grant tenure and academic rank is perhaps the most important single organizational lever. It signifies that mission-oriented interdisciplinary research is legitimate. This is a complex area, however, since tenure in an interdisciplinary unit puts the individual in institutional isolation -- that is, it is not meaningful to people in other universities. The ideal arrangement would be joint appointments with disciplinary departments where the interdisciplinary unit would be able to give tenure and rank in the faculty members' disciplinary department. Needless to say that is hard to sell, and an intermediary position is the more widely used joint appointment which involves negotiation and ad hoc arrangements for each individual. This is a cumbersome and insecure situation for the individual involved. In this situation the interdisciplinary unit gains some negotiating leverage if it has the capacity to grant tenure on its own.

The need for an appropriate set of physical facilities can be viewed as a tactical as opposed to a strategic instrument. Yet one can have all the needed capabilities to reward and if the appropriate physical facilities do not exist the other capabilities will go for nought. I hope I have emphasized that research aimed at applying science to complementary problems in the university setting requires substituting organizational arrangements for what would be more easily done by an adequate theory. In simple terms, the extreme difficulty is the creation of a team product when the team includes an engineer, a physicist, a biologist, a lawyer, a political scientist, and a philosopher. A physical setting that forces interaction among representatives of foreign nations (e.g., engineers and sociologists) each with its own unique conceptual system, culture, and language are essential. In practice the research we are talking of is committee research, and in the end it requires the ability to obtain consensus from interdisciplinary teams. Only continuous organized and chance interaction will provide that.

Finally, during any intitial period, organizations such as I have been discussing need all the reassurance they can get. Recognize that the people who are involved in such activities are more aware than any of their political outside critics that they don't know what they are doing. If they are the normal -- less than genius scientists who have been programmed to pursue normally productive box-filling research careers -- they will be plagued by continuous self-doubt. Doubt which their interdisciplinary organizational situation will probably require that they repress. Therefore, the public statements of support by a university president or provost are important elements of reassurance for the people involved in interdisciplinary research. Further, such statements help hold potential adversaries within the university at bay long enough to give the organization a chance.

These four elements of support give some small chance that a university group may produce potentially useful research aimed at helping resolve social problems. It is of course, part of the conventional wisdom that most research aimed at applying science to social problems never gets further than being printed and filed. Organizations (whether in universities or elsewhere) which attempt to apply social needs, must recognize that their activities fall into three phases. The first phase involves continuing contacts with those who have a vested interest in the problem area and may therefore be able to use the research. As noted in the first part of this paper, early contact is necessary for two reasons: (1) the perception of the problem by vested interests is central to designing any effort to apply science to the problem; and (2) even were these perceptions known, the research team must start early in establishing credibility with the interested parties. Professionals in any problem area will be particularly skeptical and predisposed to reject work done by a group of university researchers unless early and continuing efforts are made to seek the professionals' advice and keep them informed of what is going on. Nothing appears to contribute to the credibility of such research more than letting both the vested interests and professionals see every fumbling and stumbling move in the research effort. Minimally, it serves to convince potential users that the research team did not start out with a set of biases that it then collected evidence to support. It did not develop a brief.

Phase II is, of course, the research phase, which has been the predominant focus of my presentation.

Phase III is the utilization phase and is intimately linked to Phase I. With few exceptions, utilization of the research requires self-conscious and continuing efforts to communicate with the potential users. This is, in fact, a time-consuming and expensive activity. It means the applied research organization must have people who are available and on call at the moment when the problem is being addressed by society. It also means the investment of considerable effort in developing efficient and useful lay presentations -- something most academics find very uncongenial.

I believe I have described an organization that is not very compatible with the present structure of the university. Few such organizations will exist without self-conscious government intervention.

Requirements for Organizational Change

Numerous university administrators indicate a desire to create such problem-oriented organizations, however, they have little capacity to reorganize within their institutions. The only practical option has been to create new organizations -- which is what I'm calling for. New organizations are not a very real option, however, in an era of tight money where universities are still struggling with the financing of organizations created in response to past project grants which have now dried up.

At the present time, government research support for applying science to society's problems appears to elicit the worst results. First, institutional support has been explicitly ruled out. Only project support is available and there are no obvious mechanisms which regularly provide support for planning interdisciplinary research. In established disciplines, research progresses incrementally and builds on the past. Interdisciplinary research involves starting from zero. Quality interdisciplinary research requires expensive planning and a high start-up cost in assembling a research team. Failure to provide support for such planning increases the chances of failure.

In the absence of such support, but with both growing faculty interest and declining support for traditional research, there is a scramble in universities to put together ad hoc research groups that meet the minimal requirements of the funding agencies. These ad hoc groups seldom have the characteristics necessary for success. They have neither the rewards nor the sanctions necessary to insure cooperation among people from diverse disciplines. Without such levers the likelihood of producing useful quality work on time is very low. Such jerry-built efforts in universities have the dual disadvantage of not meeting the government's needs while creating stresses and problems for the university.

In summary, if the Federal Government wants science applied to the changed social problems of our society it will have to come to grips with the need for appropriate organizations. In universities these organizations can only hope to succeed if they have stable continuing institutional support. In practice the requirement is for some organizational system analogous to the land grant system.

Apparently, the conventional wisdom in Washington has ruled out institutional support because once committed such support regularly becomes nondiscretionary. Project grants by comparison have very attractive self-destructing characteristics. They relieve government of the burden of proof, and the associated political pain of withdrawing support. One can only sympathize with such instincts in an increasingly inflexible bureaucratic society.

Nonetheless, government must address the trade-off squarely. The point may be most quickly identified by recognizing a distinctive theme of recent administrations. It is, that high-quality performance in the public sector requires appropriate organizational arrangements. Major efforts have been devoted to institutional reorganization based on the belief that organizations must be designed to achieve specific objectives.

Only with institutional support can universities create organizations capable of applying science to social problems. Without such organizations, universities will make only limited contributions.

In practice, the choice between institutional and project support is not as stark as it may appear. First, universities will, for pork barrel reasons if no others, continue to receive federal R & D support. Under project grants, we can expect a pattern of continuous disappointment on both sides. Second, the danger of government interference that many university people see in a government program aimed at building new organizations within universities, misunderstands both the past and the present. Universities would be far better served if a clear choice as to whether this research should be done were framed for them.

A Network of University Research Organizations

Specifically, Congress should establish a program which would create at least one major problem-oriented interdisciplinary research unit in a university in every one of the 50 states. The program should have guidelines requiring proposals from interested universities which indicate their problem focus and internal organizational pattern. Faculty in these units should be insured all the prerequisites given to people in traditional departments.

Such a network of university research organizations would provide a professional career system for people doing problem-oriented research. One can reasonably expect that over time the system would assume the same characteristics of arteriosclerosis that affect other systems. This is a cost that will have to be assumed if universities are to apply science to social problems.

However, certain actions can be taken to protect against early hardening of the arteries. The new university organizations should have certain general characteristics. Their faculty should be organized in task force, or project teams. Certain program foci, such as energy may warrant a relatively longer-term organizational arrangement. However, no component of the research organization should be allowed to exist for more than five years. Only a positive decision to recreate it would see a program last longer. The objective is to make sure that the specific research components or task forces are continually reconstituted around contemporary problems.

These organizations would clearly be more dependent on organizational managers than is true throughout the rest of the university. Although the new organizations would have tenured faculty, their directors would have responsibility for moving people among research projects.

The organization should provide a home for interdisciplinary projects generated by faculty both from within and outside the organization. Therefore, research would be organized around both specific projects and slightly more permanent programs.

The overall research organization should be reviewed and critiqued every five years by an outside review team with an equal number of members selected by each of three groups: the faculty of the research organization, the university administration, and the federal funding agency. The criteria should be the quality and utility of the research.

Although it is not, at present, a major motive for establishing interdisciplinary research organizations, they should have a stimulating affect on the university. The research carried out should be immediately transferred into the undergraduate curriculum, in the form of elective courses available to students across the university. These courses should be taught by the teams that have carried out the research. This pattern overcomes the organizational, language, and most importantly the lack of research base which regularly kills interdisciplinary problem-oriented courses. The absolute maximum number of times any one of these courses should be offered would be four times over a two-year period. Like the research teams they should not be allowed to become a part of the continuing curriculum.

The legislation I propose for establishing a national system of university-based organizations for the purpose of applying science to society's problems should authorize a first year expenditure of $100 million.

SUMMARY

In summary, I would note that federal agencies are major contributors to the difficulties associated with doing interdisciplinary problem-oriented research. In the Washington agency context there is a profession of need for interdisciplinary research, but an unwillingness to run the risks that are inherently involved. The reasons are understandable, since without a guiding paradigm, policy-oriented research can easily become little more than unsubstantiated special pleading. The agency response to the fear is to substitute methodology for the missing theory or paradigm. Repeatedly the agencies escape to such methodologies as cost-risk-benefit analysis or delphi or one of ten thousand types of modeling. The normal pattern is to monitize all the variables and grind them into a predetermined model. This allows the values to be hidden one level below the surface. It also insures that a reasonable estimate of the result can be had before the research is undertaken.

In the absence of the substance provided by theory, it is politically useful to have at least the form provided by a predetermined methodology. While this approach is understandable, it has the effect of a straightjacket on experiments with efforts to apply science to social problems.

FOOTNOTES

1/ Kash, Don E., et al. Energy Under the Oceans: A Technology Assessment of Outer Continental Shelf Oil and Gas Operations (Norman, Oklahoma: University of Oklahoma Press, 1973), p. 107.

2/ Price, Don K., Government and Science (New York: Oxford University Press, 1962), pp. 1-31.

3/ Gaus, John M., Reflections on Public Administration (University of Oklahoma Press, 1974), pp. 1-19.

4/ Lindblom, Charles, Intelligence of Democracy (New York: Free Press, 1965), pp. 137-164.

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COMMENTARY

Joel Snow
National Science Foundation

One of the points that was made last night was that a problem with interdisciplinary problem-oriented research is that it is team research and it is thus sometimes very hard to get that thing that is most important to many scholars -- and that's called credit. Don's group has gotten a lot of credit for its fine work. This belies the final line of the quatrain from Omar Khyamm in which Omar the Tentmaker says he'll take the cash but let the credit go. In the field of technology assessment, Kash has gotten credit for some truly outstanding contributions.

I've pulled out from the summary of Don's paper, the five characteristics that he identifies; that I would say could be called the ones that are essential for an effective technology policy-research activity. Here I want to make a little intervention in language. A lot of people talk about science policy. A lot of people talk about science and technology policy. Some people talk about technology assessment. One of the pollution problems we have in this field is semantic pollution. We see it in almost every commentary -- a lot of confusion about terms.

Chris Wright tried in his paper to straighten all that out. I notice, although Chris did a pretty good job of delineating those things, there hasn't been an overwhelming adoption of his set of categories. I think what Don has been doing has been called technology assessment. Even though an old friend of mine in Washington really coined the term technology assessment, it may be now time to bury it since it is used in so many different ways. I think it's technology policy we're really talking about. This is an exceedingly important activity and there are some key characteristics which Don brings out in the written text that I'll just briefly reiterate.

The first is that you've got to have substantive knowledge to bring to the problem, this is self-evident and essential.

The second is that you need to have a disinterested position in order to have credibility in the various communities that you're working with.

The third is that timing is crucial. That can't be overemphasized enough. Don's antischolarly remark that he would trade quality for timeliness is exceedingly important because getting a 100% correct answer five days after the decision has been made is worthless. You have to bring the best information to bear on the circumstance when that information is needed, and sometimes it is needed at several check points along the way. As Don's detailed paper and as the other work that he has done on this have indicated, the whole process of outer continental shelf leasing on which that first study of his was focused, had a lengthy sequence of places for research findings to be involved -- the Council on Environmental Quality, the industry, congressional committees, and so forth -- and timeliness in such a process is crucial. In fact, if that study had been funded a year later or a year earlier, I think we could hypothesize that the use of the research, and its impact. would have been very much different.

The fourth point is that there needs to be a ready availability of the project group to inform and advise the people that are interested in the results.

Don's final point, that I think I take issue with a little bit, is the matter of providing recommendations or specific concrete proposals. Specific concrete proposals are great, if in fact they fall out in a fairly straightforward sense from the analysis. In the OCS case, they really do fall out. They hit you in the face when you work through the problem, and it is evident to anyone what some of the obvious proposals are.

In another case that Don's group has worked on, which involved a comparative analysis of fossil energy sources, providing recommendations is a little more problematical. There are quite a number of other situations that I'm familiar with in which if you forced yourself to come up with specific recommendations and made that a sine qua non of the process you could force yourself into premature judgment.

One should let the punishment fit the crime and provide recommendations or cost-benefit alternatives, options or whatever, depending upon what fits the situation. Sometimes, however, cost-benefit analysis is the last refuge of the bureaucratic coward, for whom the obvious steps to be taken are uncomfortable or unpalatable, or are used to justify an action when only part of the relevant data are considered.

THE COMPARATIVE ADVANTAGE OF THE UNIVERSITY

Let me turn to a derived set of comments from Don's five major points. Those have to do with the comparative advantage of the university in policy-oriented research. In several remarks I've made earlier in this meeting I've seemed to imply that if it is so hard to get interdisciplinary groups going on things that are new to scientists in universities, then why bother? Why not let the commercial research organizations -- SRI, Batelle, RTI and so forth, do the job? They have competent professional staffs. They're accustomed to respond to RFP's and to go through the rest of the contract ledgerdemain. Much could be said for that. Those institutions have a very important contribution to make and on a lot of types of large-scale planning studies you've got to have their high-intensity capability -- the ability to really produce a product fast. At the same time, there is an old saying in government that "if you want it bad you'll get it bad." That is often proven out by some of the forced draft studies done by the contract research firms.

I think the comparative advantage of the university group relates directly to the central points Don made. First of all in a university you have a reasonable assurance of being able to assemble a group with high scientific and technical capability. With some of the other organizations the people who actually do the work may be very different from the people who sold you the job. Without naming any names, it just sometimes happens, and I think everyone in the research funding community knows, that the very impressive group of people at the site visit turn out to also be committed to seven other projects. The high scientific and technical capability in universities and their network of contacts in the science community is valuable.

The next item is detachment from politicization. It might seem a strange thing if you think back to 8 to 10 years ago when universities were hotbeds of trouble, but we still think of the university as detached from politicization. In analyzing socio-technical questions, universities are a lot more detached than are the companies that have a major stake in getting further business studying the same questions. People in universities, more than people who have committed themselves to other types of careers, have somewhat of a propensity to tell the truth. Or to put it differently, the academic intellectual community exerts some powerful sanctions against people who don't tell the truth and demonstratively don't tell the truth.

One of the very worst things in any kind of technology policy-analysis is incorrect information, whether you call it lies or whether you call it mistakes. The existence in the university of that set of severe internal penalties for a falsification provides a very important element of quality control that aids the credibility of a process. The independence and integrity of the academic setting is thus an essential strength.

The last point I'll mention is the ability in the university to involve young people. The younger, less jaded, less tired people who have not previously really had a position on the subject can provide valuable insight. They generally have not been involved in OCS oil or oil shale, or uranium availability or whatever. These are that wonderful breed of individual whom I think of as those bright young graduate students. When we've spent a few years in the federal bureaucracy we sometimes wonder how we could get some bright young graduate students in our organization so that we don't all dry up and blow away intellectually. I don't know what the situation is with graduate students in Don's program, but I know if I were doing his job I would very much want to have a lot of students involved. Not just graduate students, undergraduate students as well. People who read what you write and scratch their head because they are just getting educated and they are learning that they have to ask the teacher questions. They say, "Well, why did you say that?" When really why you said that was that maybe you couldn't think of anything else to say and didn't think the question through very well.

I think among the advantages to the university, again for that matter to the researchers, are the experience of involvement in an important public problem and the contact and interaction with larger publics than academics usually come across -- such as the National Resources Defense Council, the Sierra Club, the oil companies, and state and local government.

THE NEED FOR CONCISE RESULTS

There are difficulties in providing a full, scholarly analysis of a problem -- and in getting decision-makers to read that analysis. Don made a comment about the need to make recommendations. It is important to recognize that you are dealing with a group of people who have time only to read 10 pages, not 300. The recommendations in a study can provide a concrete example of what the runout of the analysis is and one way of dealing with it that busy people can focus on.

That is an awfully good thing to do. It may be that if the sponsor is scared of reality then in the formal report you may have all these nice alternatives and options and cost benefits and so forth, but in your pocket you have another sheet of paper that lists the recommendations. You then whisper in the decision-maker's ear and say, "Say, you know there are some recommendations if you want to look at them yourself privately." Then he can take the piece of paper and he can go to the men's room where there are no microphones or anything and he can sit and read them and find out what the real recommendations are.

You know, people often don't have time to read ten pages and this is one of the most severe problems of government today. When Don said ten pages, Vaughn Blankenship, who has worked in OMB, leaned over and said, most people will only read three pages. Well, that's wrong! They'll only write three pages. They'll only read one page! When I first went to work for Guy Stever doing planning and budgeting and so forth, one of the things he told me was that he didn't want to see any reports coming into his office that didn't have a one page cover sheet that told him everything that was important in the report because he would never have time to read the full report. And, in two years there has been one change. He wants it in half a page. So, there is a lesson there of some kind, since Dr. Stever is a highly responsible and conscientious official. But he just doesn't have time.

PROGRAMMATIC SUPPORT

My last comment is about institutional support for policy-relevant research. I don't know whether it is the words or whether it is the concept, but institutional support expressed as such is never going to get anyplace. It's never going to get any place because it connotes support for a whole set of activities that have really nothing to do immediately with getting the particular type of job done that the sponsor is interested in at the moment. Institutional support, for better or for worse, connotes to most people a support of an institution as a whole. It died in the National Science Foundation years ago when it appeared that institutions generally were turning out many more Ph.D scientists than were needed. By pumping money generally into university growth we appeared to be exacerbating what was already perceived to be a problem. It would be pretty crazy in fact to have one hand stimulating the production of Ph.D.'s and another hand setting up special programs to find jobs for excess Ph.D.'s.

On the other hand, I think the essence of the points of the need for continuity, of the need for stability, of the need for a degree of insulation from the short term pressures of pernicious grantsmanship, are well taken. How can these goals be achieved? What I think is needed, perhaps, could be called programmatic support. This would be support for a problem-oriented program that has a theme or series of unifying themes and has a relatively long time perspective. This would be designed to build and maintain a capability for problem-oriented research around a series of general topics, and it is the capability rather than the institution building that is essential. That, I think, is the target, and such support could provide the continuity and stability needed while continuing the discipline of a relationship with a programmatic and demanding sponsor.

* * *

DISCUSSION

Don E. Kash: I've tried to overstate my case for emphasis. I chose, for instance, institutional support because I know it causes everyone in the system to go "aaahhh." But, basically, what I'm talking about is support for organizations that do this kind of research.

Now, if I come to the question of methodology, I don't mean to suggest that any one of the numerous methodologies doesn't have some value. What I have found, however, in dealing with granting and contracting agencies is that at any point in time there is a program manager who is addicted to something called cost-risk-benefit analysis. And, the RFP is probably written in those terms. If it is not written in those terms the information discussion will be in those terms. Although I know some examples of what people call cost-risk-benefit analysis, I am still not sure what it is.

The second point I want to make applies particularly to the area of technology assessment. The National Science Foundation went through a phase when this buzz word came in, and they put their money into developing a methodology for it. That phase was eliminated when, after spending some substantial amount of money for developing technology assessment methodologies and nothing was produced, the decision was made to simply fund substantive studies. If you are going to do whatever this is you just do it, you don't study methodologies.

In substance we've got a beautiful symbiotic relationship between the universities and the Federal Government. We have trained people in universities and many of the best of our products have gone to whatever party line they were trained in. The problem with the bureaucracy today is not so much a problem of corrupt politics, it is a problem of mad academic lines. So that whatever bureau-agency program office you happen to deal with will be probably populated by people that have some addiction to one or another of these frameworks, forms, methodologies, schemes -- whatever the case may be.

And, my point is that if you start with the assumption that I start with, if you're going to do policy research, the first thing you've got to learn is something about the substance of what you're studying. I'm always reminded of this story about the tropical country's legislature that was under a great deal of pressure because there was a malaria epidemic, so they simply outlawed malaria. They didn't spray any DDT or put any oil on any swamps or drain any swamps. If you're going to do policy research you've got to recognize the substantive boundaries and once you begin to do that then you'd better use good judgment and prudence and forget about trying to jam policy-oriented research into some abstract scheme. I say that and I probably over say it. Many of these methodologies have very real heuristic value. They're useful. They identify things. They stimulate thinking or at least they have the potential for stimulating thinking. Yet all too frequently in practice, it is my judgment, what they have become are intellectual straightjackets.

Ann Macaluso: This is more of a comment than a question, and I am not certain exactly how to say it. Perhaps it follows what Dave Rose is saying. There is enormous uncertainty, it seems to me, in everything that we've been talking about in the past few days here. We wouldn't be here if there weren't. Uncertainty is the name of the game, and having the ability to live with doubt and anxiety and all the rest of it is important.

What troubles me most is the attitude that I've heard expressed over and over again since I got here: what is important is tenure and security -- that people are cowards and can't cope with the uncertainty and anxiety. I think the times demand old people (like some of us with gray hair that is covered up) to do exactly the same thing. And, I can't help but be extremely troubled ever since I got here by feeling that I must be a damned optimist: all the rest of the world knows something that I don't and, therefore, I'm terribly naive.

Joel and I know of one study in which a member of NSF was fired and a member of another department was given a departmental achievement award for the same study. It happened to be an interdisciplinary study on which I was involved. Some people do get fired for those things and some people who do get fired go on to bigger and better things.

I suggest that we all ought to stop looking at ourselves as though we had limited, very short life spans and had to spend them in exactly the same place, doing exactly the same thing we've always done, and take a look at the subject matter of this session and begin to dare a little bit more.

Joel Snow: The point may have been too strongly stated, but there is a basic difficulty that makes me wonder to some extent about all these fine people who are products of all these scientific discipline straightjackets. I guess the problem is they are human. When a relatively junior staff person knows that the recommendations to come out of a project he is responsible for are likely to enrage his boss, he is probably not going to encourage that those things become too visible.

Suppose you were to study the following question. It is a policy-oriented question dealing with very much the same type of technology that Don has been involved with. Suppose you were to look at the consequences of the administration's proposal to deregulate natural gas at the wellhead so that the intrastate price of gas (which is 10 times that of the interstate price) would become in effect the interstate price. That would provide an enormous incentive for drilling natural gas formations and extracting lots of existing natural gas. It is possible that we don't have any shortage of natural gas in actuality right now. Now that is very interesting. If you ask what would happen to the price of natural gas -- obviously it would rise (there is an economic model for this, that has actually been funded by NSF, by Paul MacAvoy who is now in the Council of Economic Advisors). The model shows you, roughly speaking, that the price of gas would rise to about 75% of the current interstate value. It would undoubtedly stimulate a lot of exploratory investment in the gas industry.

Suppose you were to then ask the question, what about the administration's additional proposals to stimulate the development of synthetic fuel-production technology by a very large-scale program of loan guarantees in order to encourage the large companies to build synthetic fuel, coal gasification, and high BTU coal gasification plants. And, ask yourself whether all of those cost analyses that show that with a certain level of subsidy those plants are going to be able to compete economically will be true once you've deregulated gas at the wellhead.

What you will then probably find is a very unpalatable answer. If you were doing a policy study for the Administrator of ERDA, and produced a study and the consequent recommendations that the loan guarantee program be junked, you might well be fired.

That's a hypothetical case study. I used that example for the very reason that that particular one has not taken place but there have been many similar situations that I'm aware of that simply indicate that the vested interest of the high-level bureaucrats in the particular political policies exert an enormous coercive force on the integrity and the professional respectibility of the middle tier of people who in fact monitor the projects and make most of the arrangements for them. And it is a very unfortunate situation. I've worked under three administrations, Johnson, Nixon, and Ford, and the same kind of thing has been true in my experience. I don't know if it is always true.

Dan Alpert: Don, I wonder if I could make a few remarks that are complementary to and supportive of your comments about the importance of the knowledge of the substantive area and some other remarks concerning methodologies. In your paper, as in many papers, there is reference to the need for something like the land-grant organization by which I think you mean the organization of the College of Agriculture. And, indeed, within living memory of some of us there is an experience within the university of successful interdisciplinary research in the College of Agriculture. I think it is interesting to recall some of the following attributes of that research.

First of all, people were hired, given tenure, and organized in departments called Animal Husbandry, Dairy Husbandry and so on. You would recognize that animals are horses and pigs, and dairy, and so on, and by their very nature you'd know what the scientists were working on. They were working on the problems of the dairy farm, the farmer, generally. The people who were hired were biochemists, physiologists, economists, and so on -- hired into a department and they agreed to work in a place called Animal Husbandry.

I think it's important to note that while it may have been important that they were good biochemists or good economists, I think a study of those activities would suggest that even more important was their knowledge of not only the technological problems but the cultural context in which whatever product they were delivering was going to be used. Virtually without exception these people came off the farm and had lived there from birth -- both students and faculty. When they reported the results of their activities it was rarely, if ever, in the form of a written report. It was as timely as it could possibly be. And having grown up on a farm myself I know that my dad would call up Professor Jones (he called him Professor Jones, but they had a very personal relationship), and Professor Jones would come out to the farm. I think these experiences were very characteristic of one of the few cases of interdisciplinary team research which had organizational support, and fit into the organizational framework of the university in terms of organizational tenure and all that stuff.

I think the final comment I'd like to make is that at sometime in the history of these departments, mostly after WW II, an interesting thing took place. We changed the name of the department from Animal Husbandry to Animal Science; from Dairy Husbandry to Dairy Science. The disciplinary values of the academics on the north side of the College of Agriculture took over and the subjects that people started to study were not problems of animal and dairy. I'm not making a case that this shouldn't have happened, but it is an observation that the people working in these departments now work on cell biology, nutrition, microbiology, etc., and I defy anyone, even the experts, to go into these departments and tell which is which. They retain their organizational structure. There is still a department of Animal Science and of Dairy Science and there is virtually no reference to the animal out of which the cell structures were taken.

> " ... the real problems in implementing the Sustaining University Program lay not in Washington but on campus. The Webb leverage -- money, buildings, agreements with university presidents -- was insufficient, given the nature of the institution he was seeking to change ....
>
> ... The 'bottom line' for the (National Science) Foundation was clear. The RANN program was not to provide institutional support to universities. Not only was such support not required in a period of manpower oversupply and underemployment, but it would limit the Foundation's ability to 'direct' research towards needs other than those of the research-producing community."

L. V. Blankenship
and W. H. Lambright

UNIVERSITY RESEARCH CENTERS:

A COMPARISON OF THE NASA AND RANN EXPERIENCES

L. Vaughn Blankenship
Planning and Policy Analysis
National Science Foundation*

and

W. Henry Lambright
Associate Professor
Syracuse University*

Both the National Aeronautics and Space Administration (NASA) and the National Science Foundation (NSF) have had programs aimed at applying research to national needs. The Sustaining University Program (SUP) of NASA actively sought to relate universities to societal problems. 1/ The Research Applied to National Needs (RANN) program of NSF has pursued a variety of different strategies in its efforts to tap the brainpower of university researchers. While it is arguable as to which program has had the better results, it is worthwhile to compare the two programs for the lessons they shed in government-university relations. Both intended to do more than merely support researchers doing traditional disciplinary-oriented basic research. They sought to link portions of the scientific community into a research system whose output would be information and technology of applied relevance to problems of national concern. In doing this, they had to consider how to implement their goals and chart the place of interdisciplinary, problem-oriented research in their programs. The university environment with which they were dealing was characterized by a number of factors (a) a tradition of research autonomy in which ideas are generaged by members of the research community; (b) an organizational -- and power structure -- dominated by disciplinary-based academic departments; (c) an organizational value system stressing multiple goals, of which research is only one; (d) a production system scheduled, largely, around educational activities and the academic calendar; and (e) an organization with few direct links to potential "users" of applied research.

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* The views expressed in this paper are those of the authors and do not represent the opinions of the institutions with which they are affiliated.

THE NASA SUSTAINING UNIVERSITY PROGRAM

NASA had and continues to have a vast program of research involving universities. The bulk of NASA university money goes to traditional project support of varying scale. This entails work on aeronautical and space problems. While this work may be said to be applied to national needs, the needs are really those of the space program. Another much smaller segment of the overall NASA program has used universities to train NASA's own midcareer personnel. At the same time, NASA makes facilities available to university personnel through summer faculty fellowships. Again the emphasis is on research applied to NASA needs. There are other programs, such as one concerned with black colleges, but most of these are far removed from the kinds of issues posed by this conference. A program on target with those concerns and one that stands in contrast to other NASA efforts involving universities is the Sustaining University Program.

No longer in existence, SUP lasted from 1962 to 1970. Its orientation was toward getting university research applied to needs that were broader -- more "national" in a sense -- than those represented by space missions alone. Indeed, the very breadth of the program's goals made for confusion both inside the agency and among the recipients of NASA grants as to what the agency really wanted. It was not clear that "the agency" was of one mind. What NASA Administrator James Webb wanted did not always appear to be what his middle managers, charged with implementing the program, wanted. And what Webb or his underlings desired was not necessarily what the universities would deliver.

Goals

SUP's problems started with the multifaceted nature of its goals. The program had at least four. One was to increase the number of Ph.D.'s in space science and related fields. This was a direct response to a concern at the time that the United States was being outdistanced in science manpower by the Russians. A second was to strengthen universities with an existing excellence in space science. A third was to increase the national base of competence in university space science through helping to upgrade second-and third-tier universities. These three goals were directed at sustaining and building research competence in space-related fields. The SUP in this sense was a complement to NASA project research. These three goals clearly were related to the NASA mission. This could not be said of the fourth goal. This goal was to develop in universities a capability for responding to national, regional, and local needs in an interdisciplinary and problem-oriented manner. This objective was a consequence of Webb's desire to change the university, to make of it less a collection of individuals and more an institution or organization that could be applied to public problems.

Means

The primary means used by NASA to achieve these goals was money: money to universities for fellowships for students the universities would themselves select; money for research along broadly defined interdisciplinary areas the institution wished to pursue; and, finally, money for new buildings to house the research and NASA fellows. These buildings, or facility grants as they were called, involved no matching funds from the university, but they were not regarded (at least by Webb) as outright gifts. The facility grants went to the universities that were expected to play an especially large role in the space program, those institutions already receiving a great deal of other SUP (and NASA project) money. Webb expected a quid for the NASA quo.

In Webb's view, universities had to develop an institutional capability and this meant interdisciplinary research. Such a capability also required leadership on campus. In the view of Webb, the central leadership of the university had been weakened by traditional project grants used by government agencies to award research funds. These established links between Washington program managers and university professors, placing the leaders of the university in the role of clerks. Webb wanted to revitalize that leadership and to help university presidents to reform their universities in societally relevant ways. It was felt that a building would provide a catalyst for achieving problem-oriented, interdisciplinary research. University presidents could use "space centers" as symbols for change on campus.

The specific means chosen to bring about these results was a Memorandum of Understanding that went with every facility grant. There were approximately 34 of these memos promulgated during SUP's existence. The memo was signed by Webb and the president of the university receiving the building. The contents varied with the particular circumstances, but, in general, the memos included clauses stressing the following agreements:

First, the university would make available its total competence for space-related research. This included not just the physical and life sciences and engineering, but social science, business administration, law, and all the rest.

Second, the university would seek, in an "energetic and organized fashion," to promote multidisciplinary research on problems in space research or having to do with the organization and implications of the space effort.

Third, the university would seek ways of diffusing the results of space-related research and putting those findings to work in advancing development and solving technological, economic, and social problems -- particularly in the universities' own regions, but to some extent the nation as a whole.

The seriousness of Webb about SUP cannot be sufficiently stressed. In conversations with his staff and later with university executives, Webb again and again made it clear that he expected universities to change. He was aware of the difficulties of getting a more problem-oriented focus on campus. It was because of the difficulties, in part, that he felt it important to push SUP. He wanted to provide university presidents with the resources he believed could help them bring about the change. He was leaving to universities the manner in which they achieved their interdisciplinary, problem-oriented framework. It was up to them to decide how to move. But he expected movement. When, later in the decade, he detected little of the kind he wanted, he replaced the subordinate in charge of SUP with an individual he believed more fully understood the Webb vision of the program.

Eventually, Webb left NASA, the budget crunch came to NASA, and SUP was terminated. Aside from those directly involved in its administration, there were few in NASA who mourned its passing. Most of the agency saw SUP as a "giveaway" of resources to universities. They perceived no return to NASA -- especially from the facility grants. He was trying to achieve a new kind of university -- one that could be more useful not only to NASA but, more importantly, to other national, state, and local programs then emerging. During the height of the space program, for example, Webb spoke of developing a capability, through NASA SUP money, by which universities could be helpful in attacking the problems of the cities.

Results

By the standards by which most university programs were judged in the 1960's, SUP was a great success. In the decade of SUP's existence, approximately $222 million were spent in universities throughout the country. Thousands of new Ph.D.'s were trained. Universities with existing space-science capacity were further strengthened. New centers of competence in space fields were launched. When it came to the goals other than Webb's special objectives, SUP scored well. When it came to Webb's test, SUP did not produce as hoped. Webb felt that his money, his buildings, his Memos of Understanding, his conversations with university presidents would produce something new and different on campus.

At minimum, he expected a more coherent response from the university to societal problems, whether NASA or non-NASA related. Evaluations of SUP, however, demonstrated few successes. As one group put it in a 1968 report:

> The multidisciplinary aspect of the Sustaining University Program research grants has generally not been taken seriously by the universities. The universities perceive the grants as institutional support in a conventional sense that does not require innovation in the administration of research . . . .
>
> Little evidence was found that the Memorandums of Understanding associated with Sustaining University Program facilities grants have led to anything but talk. Usually only a few administrators within a university even knew about the Memorandum. They had not attempted to use it as a tool to induce changes in procedures or attitudes; they did not regard it as requiring them to do anything new or different . . . .
>
> The failure of the universities to respond to the explicit agreements of the Memorandums -- technology transfer and multidisciplinary research -- suggests that the SUP goals, which they contained implicitly, were not achieved. Thus, the SUP facilities program cannot claim to have developed concern for societal problems, capability for institutional response, awareness of a service role, or strengthened ties with industry and the local and regional community.
>
> The major criticism that must be made of the universities' response to the Memorandums of Understanding is that they did not try. They clearly committed themselves to make an "energetic and organized" effort to implement the Memorandums, and then did not make it. 2/

Lessons

Why did Webb fail to achieve his SUP goals? Webb attributed one reason to the lack of attention by his own staff to those goals. Therefore, he reorganized his university affairs office and replaced its leader in order to mitigate this perceived problem. It is likely that many of those who worked for Webb regarded Webb's goals as visionary. They were extremely busy trying to run a conventional university grants effort that had grown from nothing to a huge operation, virtually overnight. While they might have seen the Webb goals as desirable, it is also probable they saw no way they could implement them. So there was not the push from NASA Webb wanted. This lack of day-to-day push for reform allowed many universities to ignore the strong, but necessarily infrequent, importunings from Webb.

Whatever the weaknesses inside NASA, however, the real problems in implementing SUP lay not in Washington but on campus. The Webb leverage -- money, buildings, agreements with university presidents -- was insufficient, given the nature of the institution he was seeking to change. Moreover, that leverage was diffused by the fact that the same institutions, particularly the prestigious universities Webb most wanted to influence, were simultaneously getting money from other Washington bureaucracies that did not carry the NASA strings. It was weakened also by the way it was managed on campus. The program called for a coherent response from the university and left it up to the university to decide how it would respond. In other words, here was an effort to change universities from outside, but it was left up to people inside to choose the manner in which that reform was to proceed.

SUP was designed to place management responsibility in the university central administration -- indeed, the president. It was the president who signed the Memo -- not Webb's own officials or university professors. The presidents were supposed to manage the NASA money with the Webb objectives in mind. But the professors did not wish to be managed, and the presidents who signed the Memos had little inclination to try to manage them -- at least in the directions those Memos implied. They signed the document and then went on to the next crisis. The professors, meanwhile, split up the NASA SUP pie, went back to their departmental offices and, essentially, did as they pleased. It was business as usual.

Webb apparently did not understand that most individuals choose academic careers in the first place because they prefer the kind of work those careers imply. That work is generally done alone or with a few graduate students. It is performed within the confines of a discipline. Once in the academic system, the pressures of promotion and tenure regard working at the frontier of the discipline and being a good department man. They do not regard engaging in the kind of work that Webb's goals implied. To make professors change their ways, presidents would have to transform a veritable way of life, alter reward systems, and shift power from departments to interdisciplinary centers. That was a lot to ask of university presidents. These individuals are weak executives by almost any organizational definition. Moreover, the faculty in most universities want to keep them that way. In short, Webb tried to change the university by exerting leverage on the president in hopes that he, in turn, would exert his power within the university system. This strategy did not work.

Thus, the goals of SUP were unrealistic, given the nature of the institution to which they were applied. They were unrealistic, given the means used to "deliver" the NASA policies. They were unrealistic, especially, in assuming that just because a president signed a Memorandum of Understanding, he would take the risks to his own career entailed in becoming an inside reformer. As the top executive of NASA during its glory days, Webb undoubtedly believed anything was possible. He was able to lead America to the moon, but he could not change the university.

RANN AND UNIVERSITY RESEARCHERS

In March of 1971, the National Science Foundation established a program of Research Applied to National Needs (RANN) and housed it in a new organizational unit, the Research Applications Directorate. Dr. Alfred J. Eggers, formerly with NASA, was selected to be the first Assistant Director of the new program.

Like most such events, the creation of RANN had a history and a good part of that history focused on discussions of two central questions: How should an applied research program be organized within the National Science Foundation?, and How should it go about conducting its business? 3/ Over the preceeding four years different actors within the Foundation, the Congress, the Office of Management and Budget, the National Science Board -- the policy-making body for NSF -- and the scientific community offered different answers or, at least, emphasized different factors. The issue of the new program's relationships to the academic scientific community was, of course, embedded in these larger questions and to answer, and operationalize, them was, by implication, to specify how RANN would relate to university researchers. Nor was the issue fully resolved with the establishment of RANN. Instead these relationships evolved and continue to evolve as the program has sought to harness a portion of the talent in the scientific and engineering communities to the conduct of research oriented towards "national needs."

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Goals and the Organizational/Policy Environment

Several important factors shaped the organizational and political environment within which RANN emerged and has continued to operate. The National Science Foundation has traditionally had no "mission" other than the support of high-quality scientific research and science education. During the twenty years preceeding the formation of RANN, it developed a particular style of planning and management which relied heavily on inputs from its research constituency to assist it in setting priorities, identifying areas of science and engineering to be supported, and selecting specific projects to be funded.

This meant two things for the new, applied program: (a) it was of a "free standing" character since, unlike the traditional "mission-oriented" agency, it had no set of clearly identified end users or decision-makers whose functional concerns could provide a ready-made basis for a research agenda and (b) the dominant managerial paradigm of its parent agency was both passive and other-directed, driven largely by the values and concerns of a research-producing community located in large universities. Representatives of this community were on the Science Board and staffed most of the Foundation's previous research programs.

INSTITUTIONAL SUPPORT

There was no functional equivalent of NASA's Sustaining Universities Program. In 1960 there had been concern that the supply of scientific and technical manpower was inadequate to meet the nation's needs. By the late 1960's, the policies and programs adopted to meet these concerns appeared to have succeeded all too well and the worry was that there was an oversupply, and underutilization, of scientists and engineers. In addition, the enormous increase in federal funding of basic research during the 1960's appeared to have added a sizable increment to the nation's scientific and technical knowledge base and, beginning in the last years of the Johnson Administration, there was a growing feeling that this knowledge wasn't being utilized, that the value of the "payoff" might be disproportionate to the political opportunity costs of the national investment in R&D.

The immediate organizational predecessor of the RANN program, the program for Interdisciplinary Research Relevant to Problems of Our Society (IRRPOS), had considered making some form of institutional award to selected universities to assist them in developing capability for planning and conducting large-scale interdisciplinary research on applied problems. There was a feeling that such efforts imposed special costs and difficulties on universities given their incentive structure, orientation towards basic disciplinary-oriented research, and domination by academic departments. As in the case of the SUP of NASA, it was believed that such awards might overcome some of these difficulties by giving legitimacy and resources to more applied, interdisciplinary undertakings.

During the 1960's, NSF had established a sizable university (and departmental) development grant program with the avowed intention of upgrading second-tier universities and departments to first-class status. 4/ Some of the proposals they were receiving could be viewed as having an applied thrust to them. After all, concerns about utility and social relevance were "in the air" on many university campuses by the late 60's and 70's, driven both by altruistic concerns about pressing social and environmental issues and concerns about justifying the maintenance of large graduate programs and future employment opportunities for the graduates of these programs.

As these proposals were being reviewed for their potential relevance to IRRPOS, however the RANN program was being planned as a replacement for IRRPOS 5/ and the entire university development program was being phased out by the OMB as one of its strategies for reducing the future production of Ph.D's. Review of these proposals continued and, with the explicit approval of the OMB, seven were finally selected for funding as "fitting within the RANN program criteria" though it was generally recognized that they were disguised form of institutional support for interdisciplinary, applied research in the affected universities.

THE POLICY FRAMEWORK

The NSF-OMB interactions surrounding both the review of these proposals and the creation of RANN made the broader policy orientations clear. In periods of oversupply, the government, including NSF, should avoid exacerbating the problem by cutting back on programs which fostered Ph.D. production. New activities like those represented by RANN were a way to put underemployed scientists and engineers to work on projects with a potentially high social utility. One of the rationale's for the SUP in the early 1960's had been that the space program might, in conditions of a constrained academic labor market, attract researchers and graduate students at the expense of other scientific areas and university needs. With conditions of oversupply and underutilization, conditions which existed not only within the university but within the economy as a whole, there was much less need to be concerned about this problem and to take steps to offset it.

The perceived oversupply of basic scientific and technical research knowledge and its underutilization relative to the size and cost of the national investment in R&D led to similar conclusions. The problem was not to create more basic understanding but to put that which was available to use in "solving" national problems. This, again, would have the added benefit of holding down the production of more Ph.D.'s--which was closely tied to the conduct of basic research in the universities -- while putting underemployed scientists and engineers to useful work.

Finally, programs of institutional support were notoriously difficult to "direct," even when they were ostensibly focused on applied rather than basic research. The NASA experience as well as that of other federal agencies demonstrated that academics tended to go their own way, regardless of what had been promised, once the check was received. The NSF, with its tradition of passive, other-directed management, would be even more subject to these difficulties if it gave institutional support awards as a part of its applied research effort.

THE "BOTTOM LINE"

The "bottom line" for the Foundation was clear. The RANN program was not to provide institutional support to universities. Not only was such support not required in a period of manpower oversupply and underemployment, but it would limit the Foundation's ability to "direct" research towards needs other than those of the research-producing community. Since the problem of the oversupply of both manpower and basic, underutilized knowledge was not confined to the university, research should be supported wherever it could be done most effectively with the most efficient use of national resources. In short, research was to be funded on a project-by-project basis in a "buying" mode where the Foundation would decide what it wanted to purchase relative to some set of "national needs" and then select that group of researchers most capable of producing it in a timely fashion. In this way, the federal government's interest in promoting the optimal use of the nation's scientific manpower and basic research knowledge -- most of which had been supported by the Federal Government in the first place -- in "solving" pressing national problems would be realized.

Such a "bottom line" obviously clashed, at significant points, with the past orientations and management style of the Foundation, its concerns about the labeling off in federal funding for basic research and the contractions in the labor market for scientists and engineers, and its interpretation of the policy implications of these latter trends both for the Federal Government and the academic scientific community.

It also clashed with the way in which the university-based scientific community viewed the role of NSF and their relations with the Foundation. Under such circumstances, it is easy to understand the tensions and conflicts which have sometimes surrounded the RANN program since its inception. 6/

Organization Means and Issues

In order to achieve the objective of linking national research capability to the "solution" of various problems related to "national needs" in some optimal fashion, the new RANN program had to grapple with a number of subsidiary issues in a basically unsympathetic organizational environment. Five were especially crucial to its ultimate success or failure: (a) the identification of the "national needs" which would set the research agenda and establish decision-criteria for program design and project selection; (b) the establishment of a rationale for differentiating its activities from the applied research program in "mission-oriented" agencies while, at the same time, coordinating its efforts with these latter agencies; (c) the establishment of a process for identifying relevant, high quality research and selecting research performers; (d) the fostering of implementation and utilization of research results; and (e) the development of an evaluation process for assessing the results achieved and for feeding these results back into future program planning.

PROGRAM PLANNING AND DESIGN

In most applied research programs, the "mission" of the agency provides the parameters for the research agenda and a set of decision criteria for setting priorities and selecting specific research projects. These guidelines arise out of the functional concerns of the parent agency, its programmatic needs and, in some instances, the needs of its clients. In addition, most, if not all, of these agencies have their own in-house laboratories and research scientists who both produce research and help guide and evaluate work which is funded extramurally. The NSF had no such in-house capability. In addition, as a "free standing" research program, RANN had no functional mission which could provide it with a list of "national needs." Thus an alternative model and set of procedures for developing such guidelines was needed.

The traditional model followed by the Foundation was, of course, to rely heavily upon inputs from the scientific community itself. At one level these took the form of what the agency called "proposal pressure." The stream of incoming, unsolicited research proposals from the scientific community provided an indication of the priorities, research interests and problem orientations of that community. They constituted a measure of "demand" from the Foundation's scientific constituency. The peer review process helped to sort out this "demand" by indicating which specific projects were most scientifically meretorious. At another level, studies and reports by the Foundation's various advisory committees or by such groups as the National Academy of Sciences gave more general guidance as to the directions which NSF might pursue. The information produced by both of these processes as well as by the discussions of the National Science Board was assimilated by the program staff which was drawn largely from the same scientific community. Out of these interactions emerged a set of implicit priorities, program thrusts and specific decisions on proposals.

The IRRPOS program had leaned in the direction of relying on this model for its definition of problem areas to be researched. The general conception had been to support multidisciplinary efforts, largely in universities, on applied problems of national importance. The definition of the specific problems to be studied, however, rested on balance, with those submitting proposals and it seemed clear from the discussions surrounding the creation of RANN that such a reactive procedure was, *by itself*, unacceptable.

To begin with, it was unclear that the scientific community was organized in a way to respond effectively to a "free standing" program in research relevant to "national needs." Its division into specialities and disciplines corresponded nicely to the scientific issues and programs around which basic research in the Foundation was structured. These same divisions, however, had little meaning or content from a nonscientific problem-oriented perspective. Under such circumstances, reliance on "proposal pressure" was likely to produce a hodge podge of unrelated projects difficult to rationalize, justify, or manage -- a suspicion which, to some, seemed to have been verified by the brief experience of IRRPOS.

Secondly, reliance on this traditional model violated the "buying" mode envisioned by some who helped shape the RANN effort. It left too much up to the *producers* of research and gave too little leeway to the *consumers* of research. It also seemed to restrict the effort unnecessarily to the university-based research community. By institutional tradition, this community appeared to be poorly placed when it came to relating to the users of basic research. Furthermore, the problem which the new program was supposed to alleviate -- putting underutilized scientific capacity to work - was national in scope.

Other possible models for the development of program objectives and a list of "national needs" would involve reliance on inputs from "mission" agencies, Congress and the OMB, studies by user (research consumer) groups, and in-house staff. Generating or using information produced by each of these sources created problems in their own right. On top of everything else, each involved building linkages or making staffing and organizational decisions which differed in degree, if not in kind, from past NSF experiences.

In the end, of course, RANN inherited a number of ongoing activities like the IRRPOS projects, the "institutional support" grants described earlier, the weather modification program and the earthquake engineering and fire research programs which it had to rationalize within the new "national needs" framework. For the balance, it relied on elements of all of these models to develop its program priorities. Congress and the OMB frequently provided specific guidance as to program content and thrust. The staff, some of whom came from within NSF or from universities, some of whom came from other federal agencies like NASA, AEC, or EPA, suggested priorities and directions based on their previous experiences and perceptions of "national needs." An interagency liaison committee reviewed and commented on RANN program plans and, at a more informal level, program managers frequently made direct contact with their counterparts in other federal, state, and local agencies. Studies contracted to organizations like the National Academy of Engineering (NAE) or Public Technology Inc. (PTI) provided additional planning input. The flow of unsolicited proposals from the scientific community gave an indication of their perceptions of "national needs."

A set of decision criteria for judging alternative program elements and national needs had emerged from the OMB-NSF interactions regarding IRRPOS and, subsequently, the establishment of RANN. By 1972, these were formalized into the "RANN criteria" for evaluating individual proposals as well as program thrusts. They were certainly of some relevance in helping to sort out the conflicting views, advice, and demands regarding a list of national needs and more specific program elements. However, this list, like all such lists, was used on a largely ad hoc basis not to compare alternatives at one point in time but to justify -- or reject -- alternatives sequentially or to provide post hoc rationalizations for previous program decisions, e.g., weather modification and earthquake engineering.

By 1976, RANN was organized into five major operating units: the Division of Advanced Environmental Research and Technology, the Division of Advanced Productivity Research and Technology, the Division of Exploratory Research and Systems Analysis, and the Division of Intergovernmental Science and Public Technology. The first four funded research projects while the Division of Intergovernmental Science and Public Technology was intended to provide a link between RANN and state and local governments and public interest groups. A major part of its earlier work in energy was transferred to the Energy Research and Development Agency (ERDA) in January of 1975. The remaining units represent, for the time being, the organizational specification of RANN's definition of areas of "national need."

DIFFERENTIATION AND COORDINATION

The relation between the applied research activities of a free standing program like RANN and the research and programmatic activities of the other mission agencies presented a two-sided problem. To begin with, it was necessary to demonstrate that RANN was not duplicating the efforts of other federal agencies as this was an issue of constant concern to both the OMB and Congress. One strategy was to argue that it dealt with some unique problem set of national needs which did not overlap in any significant way with the research concerns of other agencies. This was a difficult argument to make, persuasively, since, at a high enough level of abstraction, the goals of the other R&D mission agencies left little out -- health, welfare, transportation, criminal justice, education, agriculture, etc. Another strategy was to argue that the NSF had a distinctive competence in funding and managing applied research. Thus even if there appeared to be "duplication" in terms of broad problem areas, RANN's approach to such problems was qualitatively different because of the distinctive qualities of the Foundation, its location in the Federal Government, and its links to a particular part of the research community.

The issue of "coordination" was the other side of the problem. There was a political need to demonstrate to both Congress and the OMB that RANN was not operating in a vacuum. Beyond this, however, the concept of national needs is little more than an intellectual abstraction until it can be operationalized in terms of a set of decision-makers (a) whose demands (needs) can help determine the actual research agenda and (b) whose organization is in a position to act on (implement) relevant research results. The domestic mission agencies were obviously an important set of potential users in the sense that they could employ the results of research in assessing some of their own programs or in improving their effectiveness and efficiency.

On the surface, at least, coordination was the easier problem to solve. An interagency liaison committee was established to assist in reviewing at least some parts of RANN's programs -- actual and proposed. Representatives from relevant agencies were asked to serve as "peers" in the evaluation of proposals or on site visits. Occasional briefing on the results of major projects would be scheduled and individuals from other federal agencies, the OMB, and congressional staffs would be invited. There were, finally, a good deal of more-or-less informal interactions between program managers and other parts of the managerial hierarchy and their counterparts in other agencies.

It was more difficult to differentiate much of RANN's research programs from programs in other agencies in order to convince the skeptical observer that there was no "duplication." There were no handy institutional devices such as the peer review process or liaison committees which could fulfill this need. Instead, it depended on the development of a persuasive organizational philosophy about NSF, and RANN's, distinctive competence, the establishment of decision-criteria which, in part, reflected this philosophy, and the demonstration, in concrete situations, that the decision-criteria have, in fact, been used.

The general organizational philosophy was an extension of the philosophy of NSF. According to this line or argument, the Foundation enjoyed a unique relationship with the basic research community and, because of that, RANN was in a position to "turn on" the very best scientists in the country to work on problems relevant to national needs. The absence of a mission and a set of functional responsibilities to a group other than the scientific community was considered a plus since it meant that the organization was not committed to a point of view and a set of obligations to interest groups. This, plus the unique status of NSF under the National Science Foundation Act, insured that its research would have comparatively greater credibility in both the scientific and non-scientific community. Thus RANN research programs were qualitatively different from those of other agencies since they could generate higher-quality, more objective research as a function of the Foundation's distinctive characteristics.

Another facet of the organizational philosophy was the "balance wheel" concept. The Foundation had frequently argued that its basic research programs provided a balance wheel to other federal R&D programs by insuring that no important areas of science were left undeveloped because they were not relevant to the mission of these agencies. In the RANN context, this meant searching for national needs and programs which were either not adequately covered by other agencies or else fell between their interests. The difficulty arose when trying to prove adequacy beyond a reasonable doubt since it is both an ambiguous and politically sensitive notion.

The RANN criteria were, in part, an effort to come to grips with this ambiguity by setting up operational guidelines for selecting programs which differentiated the RANN program from those of other agencies. Concepts like "importance," "leverage," "need for federal action," and "unique position of NSF," were intended to force conscious consideration of the underlying value, distinctiveness, and potential impact of alternative choices as well as to justify subsequent decisions.

In the end, the necessity for (a) explaining how a limited set of national needs and specific program elements were identified and selected rather than some alternative set and, (b) showing how these elements were coordinated with but not duplicative of the activities of other applied R&D programs was a constant and continuing problem for RANN. It was a problem because most complex decisions occur sequentially and are based on highly imperfect information, subjective, intuitive judgments about managerial feasibility and political acceptablility, and personalistic preferences and style. Yet the political process attempts to discuss and evaluate them as if they took place in a highly rationalistic framework in which decision-makers weigh all alternatives at the same moment in time and make objectively optimal choices according to some finite list of clearly specified criteria. Failure to respond in terms of this rationalistic framework places a program in jeopardy -- at least with a skeptical audience. Unfortunately, the reality of decision-making and program building does not fit the neat, rationalistic categories of this framework or else fits it very imperfectly. Thus choices are constantly subject to challenge, debate, and post hoc rationalizations which fail to convince.

PROJECT SELECTION

The NSF and the RANN programs accomplish their work by funding specific research projects which are carried out by scientists outside of the Foundation. These funding decisions are based on an assessment of proposals submitted to the Foundation. This process, traditionally, presented two problems: insuring that a stream of unsolicited, relevant proposals came in and picking from among them those which were most deserving of support. In the case of RANN, a third problem was added: insuring that the proposals funded added up to something which constituted a coherent problem-focused whole. A managerial system had to be substituted for the "invisible hand" of the basic research community.

The structure and incentive system of the academic scientific community pretty much guaranteed a flow of relevant proposals into the basic research programs of the Foundation. It was less clear that this same structure and incentive system could generate a stream of relevant proposals to RANN. Outside of the engineering and professional schools, few academics have much interest in applied research. In basic research, problems are determined largely by the scientific community. Integration and coherence presumably emerge in the give-and-take of the intellectual debate.

The RANN program, as we have seen, sought to define problems of perceived importance to other groups. Such problem statements were seldom, if ever disciplinary in nature, a factor which reduced further the ability of a disciplinary-based scientific community to respond in the conventional fashion.

Under such circumstances, it seemed unlikely that the stream of completely unsolicited proposals from the traditional academic constituency of the Foundation would be of sufficient quantity, program relevance, and coherence to fit the needs of RANN. Consequently, the program had to *broaden* the group of potential research performers and experiment with new (for the Foundation) methods for communicating problem concerns and stimulating the flow of relevant proposals. Such broadening and communication also fit within the conception of RANN as a *national*, rather than university, program operating in a "buying" mode.

Only a little over half of the actual and estimated distribution of research funds went to researchers in colleges and universities. 7/ The remainder was split among a number of other institutions (nonacademic, nonprofit, Federal Government, national laboratories, etc.) with an increasing amount going to individuals in state and local government or private industry. The latter trend is a part of the NSF response to current congressional directives that a greater proportion of funding should go to the private sector, especially small businesses.

RANN has continued to rely heavily on the unsolicited proposal "mode" for most of its funding. In each of the years '73, '74, and '76, approximately 90% of the awards and funds were distributed in this fashion, with even a slight increase toward the unsolicited proposals over the three years. However, many of these proposals are "unsolicited" in only a limited sense of the word since there is, typically, extensive informal interaction between program managers in RANN and researchers prior to the submission of a proposal. This interaction, plus the RANN criteria and required proposal format, provides program guidance to the potential investigator. The development and use of Program Announcements and Program Solicitations for generating a stream of relevant, problem-focused proposals has been another form of directed communication to the research community. This has accounted for about 10% of RANN's awards and funding.

The traditional NSF mechanisms for reviewing and selecting proposals for funding were modified in several ways. First, the reviewing criteria were expanded beyond consideration of scientific quality and merit to include such things as problem (program) relevance, timeliness, and potential value and leverage of results (payoff). Secondly, the concept of peers in the peer-review process was expanded to include scientists outside of the discipline of the investigator as well as nonscientists. Finally, elaborate review procedures within RANN gave the management hierarchy a sizable voice in funding decisions.

Program Directors -- the first line managers -- in the basic research programs had traditionally enjoyed considerable discretion in funding decisions. This was a reflection of the belief that, since they were experienced scientists whose backgrounds and training were in the disciplines they were funding, they, and their disciplinary peers , were the best qualified to make such judgments. Thus their decisions were seldom reviewed in any systematic way above the Division Director's level.

Program Managers in RANN enjoyed similar discretion. However, given the problem-oriented character of their funding decisions and, as we have seen, its political and organizational complexity and sensitivity, it was impossible to maintain the administrative fiction that professional, i.e. scientific, expertise alone (a) qualified them to make these judgments, and (b) limited the ability of nonexperts to participate meaningfully. Under such circumstances, the managerial hierarchy could and did intervene in and make independent evaluations of their decisions.

UTILIZATION

The output from applied research programs in mission agencies can be utilized in a number of ways. If the agency has regulatory responsibilities, e.g., EPA, research results can provide the basis for standards and decision-making. If the agency provides some good or service, e.g., USDA, or supports the functional activities of other organizations or groups, e.g., health and education, results can be transmitted directly to the impacted clientele for their action. This process is facilitated further, at least in theory, by the fact that these organizational responsibilities and needs have been factored into the design of applied research programs in the first place.

As we have already seen, RANN, as a free standing research program, had none of these links with potential clients, no functional responsibility beyond support of research. For these reasons, it needed to develop a concept of utilization and to find some way of operationalizing it in the activities it supported. It needed such a concept for another reason as well. Once developed, it would help differentiate the research it supported from the basic research supported elsewhere in the Foundation.

There are two models of the research-utilization process. Though both are linear, they have different starting points and draw on different motivational sets. One is driven by the supply of basic knowledge, the other by the demand for application or problem-solving. According to the first model, an individual scientist carries out research on a narrow problem defined as important by the discipline with which his work is identified. The results of his work and that of others will eventually be picked up in some unforeseen way and used to change or improve technology and, indirectly, some social process. However, his concerns -- and responsibilities -- are bounded by the concerns of his discipline and its search for basic, specialized, esoteric knowledge. This is probably the dominant model in the academic scientific community and those parts of the Foundation which relate to it.

The second model postulates the existence of a decision-maker with a practical problem to solve. He searches around for relevant information and technologies. This inquiry may lead to some basic research but, if so, the research is problem-oriented and problem-limited. His inquiry ends when his problem is solved. Not surprisingly, this model has tended to dominate the RANN program since it was, as we have seen, implicit in the interactions and policy concerns which gave rise to RANN in the first place. The practical difficulties were: how to identify a decision-maker and specify the parameters of his problem; how to link him up with the researcher; how to know when -- and if -- the problem has been solved so that research may be begun on other problems.

At one level the search for decision-makers was implicit in the search for a list of national needs. The functional areas within which decision-makers might be located were outlined by the selected program elements. Another factor also tended to direct and limit this search -- the public vs. private dichotomy which characterizes economic theory and American political thinking.

According to this latter set of values, the Federal Government should not support research whose benefits can be "captured" solely by the private market. Such research should be supported by the private beneficiary rather than by public tax monies. This structure focused attention on decision-makers in the public sector or else on those private markets characterized by a large number of small firms, no one of which have an incentive, or the resources, to support significant research from which the others might benefit. It also involved the RANN program in responding to constant demands that it justify, or at least rationalize, its programmatic choices in these economic terms.

The strategies for establishing links between public sector decision-makers and researchers were varied. Individual investigators were required to identify a user in their proposal and to present a utilization plan - a convincing scheme for at least getting the information out to the user community. Representatives of user groups or agencies were employed as consultants on program design, reviewers on Program Announcements and Program Solicitations, and as peers in the peer-review process. At a more aggregate level, the Division of Intergovernmental Science and Public Technology served to disseminate research information directly to state and local governments and public-interest groups. It also supported the building of research agendas by these latter groups and the establishment of science advisory offices in selected local and state governments.

It was a little more difficult to determine when a problem was "solved" sufficiently to drop it and move on to another research area. At the national needs level of abstraction -- productivity, environment, resources, etc. -- the problems are so broadly stated that they can never be "solved". Furthermore research, whether applied or basic, can only make a small contribution to any solution which is achieved. Changes in direction at this level, consequently are determined largely by decisions in Congress or the OMB. Thus the solar and geothermal energy programs were moved to ERDA and NSF was declared to be out of the energy business -- except in certain areas of basic research. A new federal agency had assumed functional responsibility for this national need.

At the level of more specific program elements or projects the problem becomes one of evaluating a stream of research output and making a judgmental determination that enough has been accomplished. This necessitates the development of an evaluation system for providing the aggregate information upon which such a judgment can be based. It also requires an act of political will since research programs and the researchers who become dependent upon them for support strongly resist their abolition or refocusing.

LESSONS LEARNED

The primary lesson learned from the RANN experience to date is the extreme difficulty of establishing a free-standing, applied research program which is supposed to draw on the scientific and engineering talent of the nation as a whole in an agency whose primary mission is the support of disciplinary-oriented basic research and whose primary constituency is the academic scientific community. The potential advantages of such an effort are several as are the difficulties.

The location of such a program in the NSF gives a degree of objectivity due to lack of prior commitment to mission which applied research programs in other agencies may lack. Since RANN and the Foundation are not tied to any special interest groups, other than the academic scientific community and its institutions, and have no functional responsibilities other than that of supporting high quality research and science education, they have less of an obvious axe to grind. This is an important attribute in dealing with problems which have clear political, economic, and social overtones.

The National Science Foundation Act gave NSF a special organizational status in the Federal Government. One of the consequences of this is that the Foundation enjoys comparatively greater flexibility than other agencies in experimenting with grants and other funding mechanisms for the support of research. Its special access to the scientific community means that it can, potentially, attract some of the nation's best scientists and engineers to work on applied problems.

These very strengths are also a source of some of the difficulties confronted by the RANN program. Lacking any functional mission, the program has had to struggle constantly to develop a list of research priorities which have an applied focus, which are coordinated with the activities of mission agencies, but which do not duplicate them in any significant way. In the absence of any functional mission to which it can point, it has had to develop a list of criteria for trying to justify why it is doing what it is doing, why it is not doing other things. Since such program decisions inevitably have large elements of subjectivity and timeliness in them and since any formal criteria are inherently ambiguous, it is impossible to prove -- to a skeptical audience -- that the right decisions were made. Due to this, much of RANN's energy must be consumed by the constant need to justify, or rationalize, its programmatic choices and priorities.

The absence of any established constituency other than the academic scientific community is also a mixed blessing. By virtue of its disciplinary organization, incentive system, and ambivalence, if not antipathy, towards applied research, it is difficult for this community to respond, easily, to the problem-oriented, short-term view represented by RANN, views which have been imposed on it by the larger policy environment in which it operates and to which it must respond. The managerial mechanisms emphasis on users, and decision-criteria developed by RANN in response, at least in part, to this same environment are foreign to a research community use to the style of basic research funding elsewhere in the Foundation.

The RANN program's efforts to develop ways of reaching researchers and institutions outside of the traditional constituency of NSF in order to meet the expectations that it is a national program buying the best, most timely information wherever it can be most efficiently produced, leads some in this constituency to believe that this results in (a) a diminution of the quality of the research supported and (b) a diversion of funds away from economically hard-pressed university scientists wishing to do basic research. This latter view is shared by some in the Foundation who feel that money for RANN has come at the direct expense of more funds for basic research.

In conclusion, such a free-standing program is caught in a web of conflicting challenges, pressures, and expectations: a university scientific constituency which wants to do basic research or at least to define its own applied research agenda; other mission agencies who have an understandable concern about the relation of RANN's efforts to their own; the OMB and Congress which have distinctive, if changing, views about the purposes of the RANN program and are continually asking that its actions be justified or explained in some rationalistic framework. The question is: What will be the institutional adjustment between the university basic research community and the applied research which NSF will support in the future?

FOOTNOTES

1/ Material on SUP is derived, in part, from Lambright, W. Henry, and Henry, Laurin L., "Using Universities: The NASA Experience," Public Policy, Vol. XX, No. 1 (Winter, 1972).

2/ Morgan, Homer et. al., A Study of NASA University Programs, (Washington, D.C. NASA, 1968), pp. 4-6, 58.

3/ For example, Hearings before the Subcommittee on Science, Research, and Development of the Committee on Science and Astronautics, U.S. House of Representatives, Vol. II, March-April, 1969, pp. 243-252; Emilio Q. Dadario, "A Revised Charter for the Science Foundation", Science, April 1, 1966, p. 42; U.S. Congress, House, Congressional Record, CXII, 15919, July 18, 1966; and U.S. Congress, House Committee on Science and Astronautics, Subcommittee on Science, Research and Development, 91st Cong., 1st Sess, Hearings on 1970 National Science Foundation Authorization, pp. 231-239.

4/ Drew, David, *Science Development: An Evaluation Study*, (National Board on Graduate Education, Washington, D.C., June, 1975.)

5/ The status of IRRPOS relative to RANN depends very much on the organizational location of the respondant. Those in NSF tend to view IRRPOS as a progenitor of RANN. Those in OMB tend to discount IRRPOS or at least to see considerable differences between it and the concept they held of what the new program was supposed to be.

6/ For a recent example, see Committee on the Social Sciences in the NSF, Assembly of the Behavioral and Social Sciences, National Research Council-National Academy of Sciences, *Social and Behavioral Science Programs in the National Science Foundation,* (Washington, D.C. 1976). Interim Report.

7/ See Table 1 of full paper presented at the Conference, which is available on request from either of the authors.

* * *

COMMENTARY

Albert Teich
Program on Science, Technology and Public Policy
George Washington University

This conference gives me a sense of _deja vu,_ since I have just returned from a meeting in Colorado which was devoted to the topic of interdisciplinary research and the universities.* The meeting was concerned with how such research should be done for ERDA. It is clear that like NASA some years ago and NSF's RANN program, ERDA wants to use the universities. It also seems clear from the attendance at the meeting in Colorado that universities want to be used, or at least they want to take ERDA's money and attempt to make some contribution to what they see as a critical contemporary problem.

ERDA AND UNIVERSITY INTERDISCIPLINARY RESEARCH

ERDA is aware of NASA's experience with universities. Many ERDA officials and others associated with the ERDA university office had in the past worked for NASA. ERDA, however, is operating today in a different fiscal environment than that of NASA in the early-to-mid 1960's, and it does not seem to have the kind of flexibility that allowed NASA to allocate a considerable amount of money to its Sustaining University Programs.

ERDA also possesses a huge laboratory structure which spends on the order of a billion dollars a year, and which limits its potential university involvement. NASA has always had large and expensive laboratories, too, but it has generally used them more as managers of an extramural performer network, while ERDA has inherited the more in house oriented AEC tradition. ERDA's large laboratory network also distinguishes it from NSF, of course, which is precluded from doing research in-house by its charter.

Furthermore, ERDA may be distinguished in general character from NSF by the fact that ERDA has a specific, applied mission. In fact, ERDA's mission seems to be one of the chief stumbling blocks to developing the kind of university relationships that many people at the recent ERDA conference would like to see established. In many areas, ERDA is using a reductionist approach. It is laying out its plans in great detail and then seeking performers for different pieces of the plans. One should not blame ERDA's leadership for this because it is largely a product of its legislation. There seems to be no place in the agency capable of funding interdisciplinary research, particularly the kind of interdisciplinary research that is conceptualized at the university, rather than in response to a detailed request for proposals.

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* "Multidisciplinary Research in the Universities," Keystone, Colorado, April 28 - May 1, 1976; a conference hosted by the Denver Research Institute under sponsorship of the Office of University and Manpower Development Programs, Energy Research and Development Administration.

What the university office has done is to develop guidelines for institutional agreements, which can serve as umbrellas. Under such umbrellas, the program units of ERDA will fund, as task orders, separate pieces of a university project. In theory, the institutional agreements will assure that the whole of each ERDA university program will be greater than the sum of its parts. Perhaps they will; perhaps they won't. What is all suggests to me is that ERDA is not much farther along than NASA or NSF in knowing how to fund interdisciplinary research.

WHAT CAN ERDA DO?

Let me ask our speakers what advice could they give to ERDA's Office of Management and Manpower Development Programs based on the experiences of the two agencies which they examined? What are the prospects for ERDA being able to establish a meaningful interdisciplinary research program?

* * *

DISCUSSION

Henry Lambright: One point to be stressed is that the times are different in the mid-70's and 80's than they were in the 60's. The 60's were wonderful years for universities in the sense that there was a lot of money around.

As I mentioned before, the leverage of NASA on the university, the outside leverage for change, was diffused by other agencies in Washington. The broader scale, interdisciplinary work was diffused by the project work which professors could get at the universities from NASA; and, diffused by the work they could get from the Defense Department and the other Washington agencies.

I think things are a lot tougher in the 70's and because of that fact I would think that the government has a bit more leverage on universities. That probably doesn't imply leverage on the very powerful, well endowed universities, but certainly when you get below that very select and small group you get too many universities that are poor in the sense that they do not have very much money although they may have a quality faculty. I think, therefore, the times would suggest that if ERDA goes about it in the right way, that they would have a bit more chance than NASA did.

Vaughn Blankenship: I would make one more comment if I was going to advise ERDA: if they are really convinced that what they want is correct, I think the worst place for them to try to do it is in the elite universities.

What they ought to do is look for second-or third-tier universities who don't have such high estimations of themselves and who aren't affluent in the same way that the elite universities are. I think the economic conditions have hit universities in the second and third tier much harder than they have the Harvards, the Berkeleys, the Chicagos, and the Stanfords.

So I guess the one thing that I would tell them is: if you really want to get what you want, you go to places that are somewhat more marginal in terms of their status in the system of science. They should be more willing to experiment.

Allen Rosenstein: I particularly appreciate Vaughn's candor and I would like to go back to his original closing remarks. I don't want to engage in a finger-pointing contest between the university, the Federal Government, and the foundation, but I would like to talk about competition in a closed ecology and the potential effects of outside intervention.

In the university we have a closed system with three competing philosophies. There is the education tradition of the English university, the research orientation of the German university, and the applied service tradition of the United States land-grant colleges. In 1950 there was the beginning of a massive intervention by the U.S. Government, through the National Science Foundation, in support of the research tradition alone with the predictable results that this conference is a direct consequence of that intervention. Now, I do not know if we can ever go back, but I am quite sure that the marketplace for research, service, and education, can not be reestablished without bringing the outside influences upon our educational ecology, our university ecology, into some better balance than exists today.

I was very interested in the remarks of RANN. The emphasis of RANN is still in support of research, in other words, the research tradition. It is applied research to be sure, but if you look at the way it is stated it is always research in search of an occasional . . . in fact, some of the research is, where can I apply the research. But, it is still research and its only peripherally bears upon the other two competing philosophies of the university. I would like to say, again, I hope that through these meetings and I certainly commend the AAAS in recognizing that we have established an imbalance in the ecology of the university.

West Churchman: This will be just one minute and it is intended with the desire that this not be the last SUP. Sorry about that. I really think some other things have to be said. Most of us that were in SUP felt that Jim Webb was in too much of a hurry. The fact is that SUP did succeed in changing universities and also other organizations. Vaughn himself is a representative of it and a host of people in Vaughn's group are former SUP graduates. Nobody knows the extensive effect that may occur when the graduates of the SUP program finally begin to have their mark. All is not hopeless.

Joel Snow: I'll resist the temptation to dilate at length on IRRPOS, RANN, and all of those heady days. Like West remarked, this will not be the last RANN because there are many RANN and IRRPOS graduates in this room and all over the country. It is exceedingly gratifying, indeed to me, to see the many ways in which the human beings who are involved in all the projects, and had all this horrible bureaucratic confusion all around them, managed to get so many interesting things done and make important contributions to the country.

I wanted to make a comment about ERDA because I think Al Teich's comment about the ERDA plan and the ERDA straightjackets are a kind of grim reminder that some of the approaches that were taken with the RANN program and with some other programs such as the DOT university program, or rather the main body of research of DOT, illustrate a very serious fallacy that a bunch of people, however well advised, could sit in Washington and lay out a plan that is the only way to accomplish the nation's objectives. This is nonsense. The Congress was wise to mandate ERDA to develop a plan. The fact is that it is going to be enormously beneficial to everybody that they have to turn in their report card once a year and Congress gets to grade it.

On the other hand, for people to believe that all of the depths are covered in this plan is foolishness. There has unfortunately grown up a philosophy already of bureaucratic straightjacketness in ERDA that tends to insist that everything that is in the plan is correct and people should just sign up to do a particular piece of the plan or if it is not in the plan it is not worth thinking about. That is a very, very unfortunate point of view because it will tend to exclude creativity. It will tend to exclude interdisciplinary approaches. And, it will tend to exclude from this very important area of the nation's R & D program, the very kind of so-called holistic approach that we started this meeting with.

I'll close with a remark about the three institutional grants aside from its enormous institutional grants to its captive laboratories, that I know ERDA to have made. The first of the grants, out of the ERDA university programs, is, wouldn't you guess, 1.5 million dollars to MIT. The scuttlebut on that basically is that, sure, there might be more leverage in the second and third tier universities, but nobody can criticize you if you give money to MIT. The other two institutional grants that I know of, institutional in the sense that they have multiyear continuity, sizable sums of money and so forth, are, one to the Institute of Energy Analysis at Oak Ridge which is essentially a holdover from the fact that Alvin Weinberg was in town for a while, and another to, you wouldn't guess, the RAND Corporation. Whatever harbinger that is for the future, it is clear that institutional support is possible with ERDA. It is not clear whether the bureaucratic conditions will be any better than they are in the previous examples that have been discussed.

Ian Mitroff: What are the implications of the Simon Report (Social and Behavioral Science Programs in the National Science Foundation, National Academy of Sciences, 1976) for RANN?

Tom Sparrow: The Simon Report, Ian, as you know, is highly critical of the social science effort within RANN. Unfortunately, the report has a certain death wish about it that conceals some very good points the report makes. The essence of it is we must return the control of RANN to the research community or else there are going to be some dire consequences.

The report essentially suggests that we should (1) recognize RANN along disciplinary lines; (2) move away from the strategy that we have tried to use over the years of having problem selection and research priorities essentially governed by users and not be the generators of research. These are the two fundamental points that the report makes.

I think that road would lead to disaster for RANN, and I'll tell you why. This pressure means that RANN, in order to survive, is going to have to start developing a constituency, and that in itself is going to give it (RANN) great problems. What is that constituency? The universities complain, but I am convinced that a sound applied research program must involve a balance between problem orientation and user orientation and research orientation.

What must RANN then do in response to the NAS report? They must generate a user constituency. I think that breeds trouble. The reason is that what we are going to end up doing in RANN, since the line agencies don't like us -- what we do is we make them look foolish -- our constituency is going to have to be OMB and the White House. And we will soon be known, as we are in danger of being known now, as the Executive Branch Policy Research Group.

Unidentified Speaker: Well, if you read the report carefully, nowhere do they choose to define the term quality. I can tell you what it is. It means that we haven't got the best performers in terms of the academic pecking order doing our work. The reason is that the best performers don't want to do applied research. And, I don't want to do it either. It is more fun to sit around and build my little models and let them fly if I can only find a turkey to give me money to do it. As soon as the turkeys left we had some problems.

What they are doing is they are keeping the standards of quality solely in terms of quality of academic effort. That is only one of the two criteria that RANN must apply to work. The second one is usefulness.

Unidentified Speaker: Let's talk about that because I know that one very well. That report addressed one thing in terms of user quality, and that was the necessity in terms of the review process, of making sure that the universities played at least an equal role in the review of RANN users. I have no objection to that at all. As a matter of fact, I think that is a wonderful idea.

> "... what I have found from our data is that the failure to view problems (holistically) is as much, if not a greater problem among researchers as it is for policy-makers. ... researchers in the main continue to view the world as if it were bivariant and research it accordingly."
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> *Nathan Caplan*

UTILIZATION AND PUBLIC POLICY:

SOME LESSONS FROM RESEARCH

Nathan Caplan
The University of Michigan

One of the questions that I have repeatedly asked myself for the last few days is "Why am I here?" At first I began to think that perhaps my difficulty is a consequence of the lead-off presentation (C. West Churchman's Keynote Address) which left me with the sense of existential loneliness and mixed feelings over the possibility of being ineffable. The more I've thought about it, however, the more I've come to realize that my "Why am I here?" problem derives from having prepared a presentation based on interview data with 204 upper-level, policy-makers in the federal government, the results of which share very little enthusiasm with the descriptions of either policy-makers or the policy-making process as described here over the past few days. 1/

SOME EXAMPLES

1. The Nature of Knowledge Used

The concern here has been exclusively with underutilization of scientific knowledge. I think we should also be concerned with the nature of knowledge which is used. In particular, I find myself concerned with the over-reliance on information controlled entirely by the using agency and the consequences of such institutional insularity of the total utilization process upon the "intelligence value of the information conveyed."

To a large degree the knowledge used in upper-level decision-making reflects informational needs as defined by the individual policy-maker: Of 575 self-reported instances of uses of empirical knowledge identified in the study, the knowledge used in 80 percent of these instances was initially ordered at the specific request of the policy-maker. 2/

Further, this identification of information needs by the user appears to be made largely on the basis of the user's experience and intuition. Respondents were questioned regarding personal contact with social scientitists, particularly those with expertise in the substantive policy area related to the respondent's job responsibilities. The purpose of these items was to determine if there were an influential "invisible college" operating.

Upper-level governmental decision-makers appear to have very limited contact with social scientists, either formally or informally, and when asked to identify social scientists whom they personally knew, persons with established reputations are rarely mentioned. In response to this question the respondents would most often mention someone on their staff who had "...taken some courses in the social sciences."

With few exceptions, social scientists and upper-level policy-makers are not linked into networks that would bring them into contact with persons, especially outside of government, who could provide professional advice and expertise on matters related to the social sciences. In fact, the gap between social scientists and policy-makers seems as obvious as the gap between the humanities and the hard sciences described by C. P. Snow in The Two Cultures.

The funding source for 218 of the primary research reports used by the respondents were identified and are shown in Table 1.

Table 1

Distribution of Knowledge Use by Source*

Source	Percentages of Instances of Use
Using Agency (conducted in house)	51
Using Agency (extramural funding)	35
Other Governmental Agencies	8
Nongovernmental Agency	6
	100

*Based on 218 instances of utilization involving primary research reports.

Fifty-one percent of the research in these instances of use was conducted in-house. Another thirty-five percent was extramural, but funded by the using agency. Eight percent of the instances of use involved research funded by government agencies other than the using agency, and six percent involved research information sponsored by nongovernmental agencies. Thus ninety-four percent of all research activities represented in the instances of use was either funded by the government, conducted by the government, or both; and eighty-six percent was funded or conducted by the using agency.

One infrequently used information source deserves special mention. Other than research conducted in foreign countries on issues of international relations, no more than two percent of the reported used involves policy-relevant research conducted outside of the United States. Whether foreign research, such as that in the areas of health care and job development are known but deliberately not used, or whether such research is simply unknown, cannot be determined from this data. However, contact with researchers outside of the United States would indicate that a considerable amount of research produced in the United States is used in social policy-related decisions in other countries. Many foreign social scientists are trained in the United States, while few U.S. scientists are trained abroad. It may well be that the U.S. social scientist community and those who provide information to policy-makers are not as conversant with relevant foreign research compared to their counterparts outside the United States. In any case, policy-relevant work is produced outside of the United States, and either because such research is unknown or ignored, it is not used by persons influencing policy-related issues in this country. Further, my personal impression based upon participation in utilization conferences outside the country, is that the best uses made of U.S.-produced social science research, particularly the more basic research, occurs outside of the United States.

The fact that policy-makers who eventually use research knowledge, order what information is perceived needed to meet those needs based on their independently derived definition of the problem, coupled with the fact that the using agency controls the production and procurement of the information necessary to meet those needs, is disturbing. It implies a utilization system which progressively delimits the opportunity for new ideas and research finding to reach those who make decisions at the top levels of governmental power. It is a closed rather than an open system in that it does not look to the outside for help in defining informational needs and resorts to a combination of organizational arrangements that have the practical effect of guaranteeing control over the information to be used.

2. The Kinds of Knowledge Used

It has been argued that knowledge for self-exploration, values and other important forms of nonscientific knowledge should be used by policy-makers. Well, they do use alternative sources of knowledge and, perhaps more often and for better purposes than most here would suspect. In fact, I have found myself more troubled over their use of scientific knowledge than their use of alternative knowledge sources in policy deliberation.

Originally we were interested in studying only the use of empirically grounded research. But it was quickly discovered that policy makers used two types of social science information and often did not distinguish between them. They use two kinds of social science knowledge -- "hard" knowledge from primary scientific sources, objectively reviewed, and "soft" knowledge from secondary sources subjectively integrated.

From the standpoint of traditional criteria employed by social scientists to measure utilization, the amount of empirically tested knowledge use and its importance in the policy-formulation process may appear disappointing. In the absence of time 1 - time 2 comparisons and in the absence of cross national data, it is impossible to say with certainty whether the identification of 575 incidents of knowledge use at the upper level of government decision-making represents a high level of research utilization. Nevertheless, while these 575 examples represent a broad spectrum of use and come from agencies of government with diverse interests, target or client populations, and missions, these instances reveal that hard, empirically grounded information is used in "screwdriver" fashion. It is mainly used for instrumental applications, usually to monitor or to measure program inputs or outputs, but not for the purposes of understanding the relationship between them.

It is surprising, therefore, to find such wide concern over nonutilization and many persons devoting their talents to devising schemes to get more knowledge into use. Indeed, if we were to measure utilization in terms of research or tested knowledge produced by social scientists, there would be reason to argue for more use. If, on the other hand, we define social science knowledge to include "soft" knowledge in addition to the more objective, formalized knowledge traditionally produced by social scientists, we would have to consider the amount of utilization as fairly high and as playing an important role in policy formulation. Its use is conceptual, rather than instrumental and, in consequence, it plays a greater role in formulating policies and in deriving an "understanding" of their effects.

The importance of "soft" social science knowledge was particularly evident from responses to the following questions: "On the basis of your experience in the Federal Government, can you think of instances where a new program, a major program alternative, a new social or administrative policy, legislative proposal or a technical innovation could be traced to the social sciences?" Eighty-two percent of the respondents replied "yes" to this item, and were then asked to provide examples of such knowledge applications. Approximately 350 examples were given.

The policy areas represented ranged widely and were as likely to be technological or medical issues as the more strictly social policy issues (e.g., the decision not to build the SST, the establishment of water and sewer construction assistance programs, highway construction projects such as the Interstate System, the decision to go to an all voluntary army, the selection of particular diseases such as sickle cell anemia and cancer for major governmental research funding, the lead-base paint prevention program, the establishment of the Environmental Protection Agency, the GI Bill, consumer information programs, major programs to "humanize" management in government operations, revenue sharing, Head Start, manpower and development programs, etc.). All of these and many more programs involving governmental actions of considerable national importance were in some way traced by our respondents to the social sciences. However, what seemed most crucial to these decisions was the application of what has been described here as the application of soft rather than empirically tested information produced by social scientists. This is not to deny that many respondents provided citations to specific social science information, particularly research, and emphasized importance in the decision process. But such information was usually only of some instrumental importance, and the final decision, whether to go or not to go with a particular policy, was more likely to depend upon an appraisal of social considerations from the standpoint of a "social perspective," derived from "soft" rather than "hard" knowledge.

It is unlikely, of course, at least indirectly, that social scientists may have played a major role in establishing this kind of social sensitivity or ethos that this "perspective" represents. If so, they seem unaware of it and certainly fail to mention it in the mounting literature on knowledge utilization and social policy.

3. Information Processing Style

Our discussions on underutilization have concentrated largely on organizational constraints and the inadequacy of technological means for accessing data. It should also be recognized that some less obvious and "between-the-ears" factors also play major roles in determining utilization.

The ways in which policy-makers process information appear to have different consequences in determining the amount and kinds of knowledge used in arriving at a policy decision even after variables such as rank and department are controlled statistically.

Before describing these styles it is important to comment briefly on the kinds of information involved and their functions in the decision-making. While scientific and extra-scientific are appropriate and proper labels for the issues under discussion, it will facilitate understanding if we think in functional terms: Consider "scientific" as referring to matters bearing on the internal logic of the policy issue, that is, pertaining to the gathering, processing, and analysis of the most objective information available to arrive at an unbiased diagnosis of the problem; and "extra scientific" as bearing on the external logic of the policy issue, that is, pertaining to the political, value-based, ideological, administrative, and economic considerations involved.

The clinical orientation. The federal officials who expressed this style, approximately twenty percent of those interviewed, were the most active users of scientific information. They combine two basic approaches to problem-solving. First they gather and process the best available information they can obtain to make an unbiased diagnosis of the policy issue. They use knowledge in this way to deal with the "internal logic" of the problem. Next they gather information regarding the political and social ramifications of the policy issue to deal with the "external logic" of the problem. To reach a policy decision, they finally weigh and reconcile the conflicting dictates of the information.

These policy-makers use the largest amounts of social research and, probably, the largest amounts of information of all types, scientific and extra scientific, in their attempt to deal with policy-related problems.

The academic orientation. The largest group of social science information users, approximately thirty percent of those interviewed, processed information with an academic orientation. They are often experts in their fields and prefer to devote their major attention to the internal logic of the policy issue. They are much less willing, however, to cope with the external realities that confound this type of problem. Considerations of the external logic of the problem are likely viewed as a menace to the prestige and standing of their expertise. Consequently, they may be the most informed on scientifically relevant information, but they use such information in moderate amounts and in routine ways to formulate and evaluate policy issues. These decisions are made largely on the basis of scientifically derived information, with minimal use of extra scientific information.

The advocacy orientation. Comprising another twenty percent of the federal officials, this group is much at home in the world of social, political, and economic realities. Their use of social science information is limited, but when used, its use is almost exclusively dictated by extra scientific forces to the extent that they will at times intentionally ignore valid information that does not fit the prevailing political climate. Their preoccupation is with the external logic of a policy issue and the function of scientific knowledge when used in that context is largely to rationalize a decision made on extra scientific grounds.

It should be assumed that these respondents are uninformed on scientific information that pertains to their policy-making responsibilities. Nonutilization among these respondents may be best headed "Deliberate nonapplication," (i.e., they absorb such information, but make a deliberate effort to avoid application unless the information supports their position or can be used to attack an opposing position). Thus, nonutilization among these respondents does not mean they ignore the relevant information; it does not mean they fail to understand the meaning or policy relevance of their social science findings (indeed, in some instances, it is clear that rejection arises because they understand only too well); it is due to the failure of the information to suit their purposes.

It is impossible to tell from the data the degree to which a person's information processing style is dependent upon personality factors, cognitive style, or differences in the way members of the various professions are trained to use information. Nonetheless, how information is processed and applied influence utilization which, in turn, is related to the intent to remain in government. We asked respondents, "Do you plan to remain in government service?" and found a sizable negative correlation (Gamma = -.37) between their intentions to remain in government and the level of social science information: The higher the respondent's level of utilization, the more likely he planned not to remain in government.

Any number of interesting hypotheses could be created to account for this finding. But regardless of a possible explanation, at least two important implications are clear: (1) Upper level policy-makers who make the most use of social science information in their work apparently are not sufficiently satisfied with their work in government to want to make a career of it, and (2) the extent of utilization is not likely to increase in the future -- more likely it will decrease or, at best, remain the same. We can expect to find less upper-level federal executives with a clinical orientation to information processing and more with an advocacy orientation. Thus, if increased use of social science information in policy matters is a serious objective of government, means must be found either to provide greater incentives for higher utilizers to remain in government or promote utilization among the low utilizers by increasing its importance in the system of career rewards.

4. Viewing Problems Holistically

Finally, much emphasis has been placed on the issue of holism during this conference and I completely agree with its importance. There is simply no other way of viewing social problems and anyone who knows about such problems is aware that there is no way of conceptualizing them adequately without thinking in terms of multiple causation. But what I have found from our data is that the failure to view problems in this way is as much, if not a greater problem among researchers as it is for policy-makers. Our respondents continuously stressed that researchers greatly oversimplified the complexity of the problems they researched. The fact is that researchers in the main continue to view the world as if it were bivariant and research it accordingly. In consequence, what we have by way of policy-related research is an unguided accumulation of particularisticly focused studies which, in my view, are less useful than one single, well-conducted, multivariant study on the same topic.

CONCLUSIONS

Repeatedly, one hears expressed that there is need for linking the producers and consumers of scientific knowledge. The need for a different set of relations between social science knowledge producers outside of government and knowledge users in policy-making positions is quite clear, but the problem of achieving effective interaction of this sort necessarily involves value and ideological dimensions as well as technical ones.

This is not an issue that has gone unattended. In fact, there has been much attention given to training knowledge utilization experts, building knowledge retrieval systems, and experimenting with knowledge transfer groups in order to interlace social science knowledge production with policy information needs. These transfer formulations and "linkage" efforts, however, have largely been based on oversimplified interpretations of the "gap" problem, and on an overreliance on an assumed pattern of knowledge use involving "hard" information (i.e., data based). This type of social science knowledge is used in a vast number of different contexts, but only rarely is policy formulation determined by a concrete, point-by-point reliance on empirically grounded data. Although the impact of "soft" information (i.e., nonempirical) on government functioning is extremely difficult to assess, our data suggest that there is widespread use of soft information and that its impact on policy, although often indirect, may be great or even greater than the impact of hard information.

In consequence, most efforts to rely on new technologies to improve the ability of decision-makers to assess the right information, where it is needed and in a form in which it is needed, have not been remarkably effective. For example, despite the sizable investment of effort and funds that have gone into the development and promotion of computerized information retrieval systems, there is no indication that they have lived up to their promise. They clearly have not revolutioned and, perhaps, not even enriched, governmental decision making: the basic problems are nontechnical.

What is needed is to come to grips with the difficulties of bridging the perspectives of social scientists and policy-makers, not necessarily the individuals themselves. Insofar as the social scientists and user communities are comprised of individuals with differing abilities and inclinations to deal with the scientific and extra scientific aspects of policy issues, effective utilization probably will proceed best if it is pursued by a set of individuals representing different combinations of roles and skills who are located in an institutional arrangment which allows them to take into account the organizational factors affecting both the production and use of knowledge.

The precise roles played by such a group would vary substantially depending upon the availability of relevant social science information and the policy issues involved. At a minimum the group must be capable of

1. Making realistic and rational appraisals of the relative merit of the enormous amount of diversified information which abounds in the social sciences;

2. Creating accurate and concise translations of social science research to faciliate communication with the policy-setting community;

3. Recognizing and distinguishing between scientific and extra scientific knowledge needs;

4. Dealing with the value issues and bureaucratic factors that influence the production and use of scientific knowledge; and finally

5. Gaining the trust of policy-makers and obtaining sufficient knowledge of policy processes to substantially introduce social science knowledge in useable form into the policy-making process at the key point where it will be most likely to be used.

These factors require a combination of individual and institutional characteristics that is not easily achieved. The difficulties of effective utilization in governmental policy-making should not be underestimated. If, however, government is serious in its intent to promote the application of research findings for the public good, then the way to proceed toward that objective is by experimenting with various forms of knowledge "transfer" groups designed with a view toward

- Improving the capabilities of present utilization arrangements and
- Developing new institutions for the purpose of coupling scientific knowledge production with policy goals and objectives.

FOOTNOTES

1/ During the period of October 1973 to March 1974, 204 interviews on social science research utilization and policy formation were conducted with persons holding important positions in various departments, major agencies, and commissions of the executive branch of the United States government. Within the governmental hierarchy, almost all of the respondents were either political appointees immediately below cabinet rank or high level civil servants. The mean income of the respondents was approximately $34,000 a year.

The majority of respondents were experienced persons. The average time in their job when interviewed was slightly over two years for political appointees and approximately six and a half years for civil servants. The respondents were chosen from agencies which represent the entire range of governmental activities, not simply those concerned with social policy, social program implementation, social problems, or the like.

The interviews conducted by professional interviewers were carried out on a face-to-face basis. The average time required for each interview was about one and a half hours. The inter-

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views were recorded on tape. During the course of each interview, the tape was used by the interviewer to help edit and complete the written narrative on the interview form. These tapes have also proven valuable for coding difficult open-ended items. A detailed account of this study can be found in *The Use of Social Science Knowledge in Policy Decisions at the National Level*, N. Caplan, A. Morrison and R. Stambaugh, Institute for Social Research, 1975, Ann Arbor, Michigan.

2/ The term *policy-maker* is used here to refer to the upper-level decision-makers included in the study. It is not meant to imply that the respondents dictated policy, but rather to indicate that they were in policy-influencing positions. The conclusions pertain only to the influencing of policy at the upper level of the executive bureaucracy, since the utilization and application of social science information at lower levels of government were not examined.

Social science knowledge or *social science information* refers primarily to information derived empirically from the following behavioral sciences: psychology, sociology, anthropology, political science, and the multidisciplinary matings of fields (e.g., behavioral-economics, behavioral-geography, psychiatry). These terms have been defined as such in order to describe the limits of the inquiry, not to imply that there are not other fields within the social sciences.

In the literature dealing with utilization, certain important conceptual discriminations are ignored, and others are not made explicit, particularly differences between such terms as dissemination, utilization, and application. In coding for utilization, attention was limited to instances of use where the decision-maker received policy-relevant social science information (i.e., by *dissemination*) and reported efforts to put that knowledge into use (by *utilization*) even if this effort to produce an impact (by application) was unsuccessful. Thus, utilization of knowledge in the context of this study occurred when the respondent was familiar with at least one relevant research study and gave serious considerations to and attempted to apply that knowledge to some policy-relevant issue.

* * *

COMMENTARY

Saleem A. Shah
National Institute of Mental Health

My comments will be in two parts. First, of course, I will discuss some issues in Nate's very stimulating and provocative paper, but due to limitations of time only a few points will be raised. Secondly, I would like to address some broader issues relevant to the perspectives of policy-makers and program administrators in the field of social problems and the the level of state and local governments. This group of policy-makers does not appear to be represented at this conference and hence an important dialogue I feel has been lacking.

Regrettably, until rather recently many segments of the academic and scientific community have not displayed very much interest in addressing issues of research information dissemination to potential "users" of such knowledge; neither has there been much general effort to facilitate the diffusion and utilization of such knowledge. In part such response, or rather the lack of response, may have related to the different value and status attached to so-called "basic" research as compared to "applied" and "policy-oriented" research. There would appear also to have been some apprehension on the part of some scientists that they might be viewed by their colleagues as "hucksters" of some sort if they sought to facilitate public utilization of their promising findings. However, the CRUSK group at the University of Michigan has been quite conspicuous for its long interest in and devotion to research utilization.

Now to some specific points in Nate's paper. One would tend to agree with the author when he notes that the data of his study "are simply too limited" to allow confident interpretations and conclusions -- even though the findings are most interesting and further investigation is strongly indicated. Despite the fairly large number of respondents and the range of federal agencies covered, wide generalizations may be problematic, especially with respect to policy-makers and top level program administrators in other organizational contexts, e.g., in state and local governments.

In survey research one typically has to place heavy reliance on the meaning and accuracy of the verbal statements of the respondents, and these statements in large measure provide the data for subsequent interpretations and conclusions. This being the case, one must consider factors such as interviewer and interviewee biases, the response set of the respondents, and the demand characteristics of the particular interview situation. For example, in this study the investigators were obviously very interested in learning about the use of social science knowledge in policy decisions.

Responses to the question, "Do you plan to remain in government service?" are not easy to interpret since they lend themselves to a variety of interpretations. Just last night, for instance, someone cited a finding in Nate's study and used it to support a particular view or belief. The finding referred to was that the higher the respondent's level of social science research utilization, the more likely that the person planned not to remain in government. Or, to put it more accurately, the more likely certain respondents were to say that they did not plan to remain in government.

If we take the above response to the question cited as an indication of attitude toward the particular governmental position at a particular point in time, we could wonder about the extent to which the expressed attitude might relate to actual subsequent behavior, viz., did the persons stating a plan to leave their positions actually do so? One might also view the response to the same question as possibly providing a valid statement of intention. In this case, again, the validity of the statement of intent would need to be assessed in reference to the actual subsequent behavior. And, lacking information about the subsequent behavior and whether in fact it related to the attitudes expressed at the time of the interview, it is not entirely clear what meaning and importance should be attributed to the particular response.

What I am trying to illustrate, of course, is the ambiguity that understandably attaches to a lot of social science research as well as to other research on many such social issues. Hence, it is not easy at all to draw clear and reliable conclusions, nor to make unambiguous policy recommendations. Moreover, given the differing and at times even conflicting research findings, and/or conflicting interpretations of rather similar or even the same findings, it must be considered rather sound and sensible policy for public decision-makers to not rely too heavily on research findings alone. In addition, it is known that decisions regarding complex social issues involve numerous other considerations besides the findings of empirically grounded research. Indeed, if the respondents in the study accurately identified 575 "incidents of knowledge use at the upper level of government decision-making," I would certainly consider this to be a very high (perhaps even unbelievably high) level of research utilization. But, as mentioned above, I do wonder whether the demand characteristics of the interview situation may have caused the respondents to strain and to stretch to find examples that would demonstrate both their awareness and their utilization of social science knowledge. Finally, it is also known that broader social and political considerations are often very critical to policy decisions and that research findings may tend to be used to provide a more convincing and "objective" rationale (perhaps even rationalization) for decisions based on other grounds.

Dr. Caplan makes a very important point when he identifies a major need and problem as that of bridging the differing perspectives of social scientists and policy-makers. There are rather obvious differences with respect to education and training, values, roles, and differing career contingencies. These differences, along with variations in styles and channels of communication, tend to hinder easy dialogue and communications between researchers and policy-makers. One might tend to agree with Nate when he notes that the decision-making at top levels of government tends in many instances to be " . . . closed rather than an open system in that it does not look at the outside for help in defining informational needs. . . ." However, I would suggest that one should also consider the possibility that in many instances the "esoteric" knowledge of the academic disciplines may not be perceived as being very relevant to particular policy issues, as compared to the kinds of "exoteric" knowledge available to policy-makers. Also, I am not sure that the typical academic research systems are themselves very "open" in the extent to which they seek or welcome information from policy-makers and other "users" with respect to the formulation of policy-relevant research questions. Obviously, if science is to have more relevance to social needs there must be a two-way street and greater efforts on both sides to achieve closer and more meaningful dialogue between knowledge "producers" and knowledge "users." 1/

KNOWLEDGE UTILIZATION AS A MEANS

I turn now to the second part of my comments. But before I go any further let me describe very quickly the particular orientation of the Center with which I am associated, and also with respect to R & D that is expected to have fairly direct relevance for addressing certain social problems. We support both basic and applied research, but the bulk of our research is applied and policy oriented. Our work cuts across disciplines and behavioral science, social science, biological science, as well as empirical legal studies which are supported in the areas of our concern. Also we try to encourage more integrated and interdisciplinary research and training efforts to address problems that typically don't present themselves according to neat disciplinary classifications. Our assumptions are that the use of public funds earmarked for research related to social problems should be premised largely on utilitarian goals, i.e., the ultimate translation of existing and new knowledge into tangible social benefits -- and not simply for the pursuit of knowledge for its own sake. Also, that since the development of knowledge is only the first step in a long and complex process leading to applications, special attention must be given to ways of enhancing the effectiveness and speed with which new information and promising research findings can contribute to better public understanding and to the prevention and amelioration of social problems. In essence, then, we see public support for problem-oriented research as a means for attaining larger social goals, and not as as end in itself.

The above approach, I should mention, is not necessarily typical of NIMH.

Much of the discussion at this conference has been from the perspectives of the scientific community and doubtless we have tended to be rather parochial in our view of broader social needs and the special role of science in addressing such needs. There may even be an assumption that improved knowledge and technology provide the major tools for preventing and ameliorating most social problems. However, it seems not sufficiently appreciated that in many social areas the barriers to desired change are largely institutional and political, and that much knowledge sits around on book shelves and is not effectively utilized. Thus, the addition of increasing amounts of new knowledge in such situations cannot be viewed as the major need. In essence, I am suggesting that if science is to be made more relevant to social needs a much broader perspective is needed and our discussions must involve policy-makers and other "users" who are responsible for dealing with the problems of concern.

The New Accountability of Science

Let me try to provide a macroscopic view with respect to the competition for public funds. Legislators and other policy-makers probably view the situation in terms of the limited revenues that are available and the motley array of interest groups making their special case for a portion of the funds. Obviously, power and influence play a very important role in gaining better and greater access to available resources. And, up until the past few years, it would have to be said that Federal support for R & D was indeed generous - nay even lavish. Such generous levels of support and an attitude of unalloyed enthusiasm and optimism about the expected role and contribution of science for meeting various social needs may even have encouraged notions in segments of the scientific community that they had some special claim to public funds and should not be held accountable like other groups. It would even seem that at times the impression has been given, implicitly if not explicitly, that "What is good for Science is good for the Country!" Now, we might remember that when Charles Wilson made such a statement several years ago with respect to General Motors it was considered both self-centered and arrogant. It would seem that somewhat similar public reactions may have been aroused by perceptions of elitism, and that greater scepticism has developed with regard to the results that are obtained from the massive expenditures for science.

Certainly, the taxpayers and their elected representatives are correct in viewing the scientific community as yet another interest group which, like other such groups, tends to couch its own values, interests, and perspectives as being very closely related to the larger public good. Interestingly, however, the scientific community has not, at least to my knowledge, applied systematic empirical methods for evaluating the effectiveness of its own activities, e.g., for assessing the value and the rate of "pay-off" from the literally thousands of projects funded each year. For example, one could use citation indexes, assessments by recognized leaders in specialized scientific fields regarding major contributions to theory, conceptual and methodological advances, and major knowledge and technological breakthroughs, etc., to determine the percentage of all Federally supported research projects resulting in significant contributions. If some such empirical approach was used, would we find 5, 10, or 15 percent of all projects to have provided some major and useful contribution? And, given various other national needs and priorities, what would be an acceptable rate of "pay-off" from federally funded R & D? Of course, the level of funding sought has very much to do with the economic and related needs of research and academic agencies and institutions and also the numbers of scientists needing research and related supported.

In short, there is abroad in the land an expectation of and demand of greater accountability with regard to the use of public funds -- whether these funds involve food stamps, welfare benefits, farm subsidies, congressional travel to all parts of the world, or the direct and indirect support given to scientists and their institutions. And researchers who may tend to disdain applied research need to be reminded that it is the *useful* aspects of science that justify most of the financial support received from governments.

Active and vigorous debate about making publicly supported R & D more relevant to social needs have been under way in many other countries as well. For example, addressing the issue of mission-oriented R & D supported by the government, the Rothschild Report in the United Kingdom recommended that:

> R & D with a practical application as its objective, must be done on a customer-contractor basis. The customer says what he wants; the contractor does it (if he can); and the customer pays.[2]

Such a view with regard to problem oriented research very likely stems from the assessment that researchers often (perhaps even typically) operate within a value system which tends to place the interests and concerns of the academic discipline and their own careers ahead of the social utility of research. The basis and rationale for the aforementioned recommendation in the Rothschild Report is indicated rather clearly by the following statement:

> However distinguished, intelligent and practical scientists may be, they cannot be so well qualified to decide what the needs of the nation are, and their priorities, as those responsible for ensuring that those needs are met. This is why applied R & D must have a customer. . . . [3]

Guidelines from Local Government

In conclusion let me share with you the guidlines that were offered to scientists wishing to make their work more relevant to pressing social needs, by Mr. William Donaldson, who was at the time city manager for Tacoma, Washington:

> Guideline No. 1
>
> We in local government are not dumb slobs who enjoy failure. In fact, we may even know more about some things than the research community does and may be helpful in using our knowledge to make practical use of some of your ideas . . . .
>
> Guideline No. 2
>
> Save your vision of the brave new urban society for your classes and learned journals. Stick to helping us provide better and hopefully more efficient services so that we will have time and resources to look at some of the broader problems of our society along with all the citizens of our cities . . . .
>
> Guideline No. 3
>
> Studies may be the safe academic way, but they only add to our waste paper problems . . . .
>
> Guideline No. 4
>
> If you start with simple problems and solve them, maybe we will trust you when you get to the complicated ones. Managing cities is an exceedingly tricky, complicated, and risky business where mistakes cause not only immediate disasters, but contribute to the fear of any sort of change . . . .

Guideline No. 5

It does not have to be perfect to be better than what we have . . . .

Guideline No. 6

You have to know enough of our language so that we can read your instructions. To expect people who work in cities to learn the language of the technologist is not only unrealistic, but it just will not happen. We do not have the time . . . .

Once you have used these guidelines to form a map, there are many problems you can help us with by applying the skills that you have.4/

FOOTNOTES

1/ Shah, Saleem A. Some issues pertaining to the dissemination and utilization of criminological research. In, Evaluation Research in Criminal Justice. United Nations Social Defense Research Institute, Rome, Italy. Publication No. 11. January 1976, pp. 207-235.

2/ A Framework for Government Research and Development. Presented to Parliament by The Lord Privy Seal by Command of Her Majesty, November 1971. London: Her Majesty's Stationary Office, 1971. See Particularly: "The Organization and Management of a Government R & D," by Lord Rothschild, p. 3.

3/ Rothschild Report, ibid., p. 4.

4/ Donaldson, W., "Science and the public sector: the user's viewpoint. Guideposts for the adventurous technologist exploring city government land." Proceedings of the National Academy of Science, 1974, 71, pp. 2576-2578.

* * *

DISCUSSION

Paula Gordon: I have developed a theory of societal problem solving and a theory of societal problems as well. It is a very simplistic notion known as the cyclic character of societal problems. The idea is best illustrated with a concrete example, say the drug abuse problem. Assume you have a situation where some people are using drugs, and they have reached a point that either requires intervention or extensive treatment and rehabilitation. If treatment is given, then you also have a point on the cycle where the underlying causes that gave rise to this problem must be met. If they are not, the cycle will be repeated.

Now, the theory simply states that in order to solve a complex problem, it is necessary to deal with all aspects of this cycle. The tendency, of course, is to deal with (only) the most overt symptoms. This tendency is one of the reasons why we have had so little impact on our most serious problems from the energy crisis to the drug abuse problem. A comprehensive approach to the solving of a societal problem includes a number of elements. One is basic understanding of the problem. Another is the ability to recognize or identify the most viable alternatives. Attention must be given to assembling resources or the creation of training of new personnel or whatever resources are needed.

We need people with administrative expertise who are also trained in societal problem solving. We don't have this resource at all. Our institutions to train people in administration are not training people for problem solving. Administration is defined in a totally different way. Most importantly, these people must have leadership qualities, interest, concern, commitment, and be able to take initiatives to bring this whole process together. Someone in Knowledge Utilization research has done work indicating that only about 2% of the population are in the innovator category. These are the people who, in my terms, have the necessary insights, understanding, vision or whatever, to solve or significantly impact the problem.

Knowledge utilization is simply a process of creating linkages between the people who are policy makers, program implementers and the like and the people who are opinion makers and generators of innovative ideas. It is expediting the educational process to get the knowledge that does exist implemented.

This is something of a holistic approach. There is implicit in it a philosophy of change and a philosophy of life which is based on a view of knowing and understanding that goes beyond the purely empirical and rational kind of approach.

" ... (advice to) scientists wishing to make their work more relevant to pressing social needs, by Mr. William Donaldson, ... City Manager for Tacoma, Washington: ' ... It does not have to be perfect to be better than what we have.'"

Saleem Shah

V.

WORKSHOP SET 2:

RECOMMENDING ACTIONS

"A pressing question raised, but not answered, was whether the education and professional experience of scientists tends to isolate them from the social problems they study and thereby to undermine the relevance of their research."

Leonard Goodwin
and Robert Knapp

G.

UNRESOLVED QUESTIONS ABOUT SCIENCE AND SOCIAL PROBLEMS*

Leonard Goodwin
Robert Knapp

The workshop did not attempt to deal with all the unresolved conceptual questions. There was considerable difficulty, indeed, in establishing a basic framework within which to discuss the agenda -- a similar experience to that described in the summary of Workshop A. However, there did appear to be a consensus for the need to examine the framework of science itself in the course of asking about its relation to social problems.

THE NATURE OF SCIENCE

The question was raised as to whether there was a basic difference between physical and social science, and whether the latter could yield adequate predictions. No case was presented for a fundamental difference among the sciences. But a fundamental difference in the phenomena dealt with was noted. Inanimate objects studied in physical science are unaware of the scientists' activities. People, on the other hand, can become aware of the research conducted about them, and thereupon alter their understanding of the world and their actions. As one participant pointed out, physical science might be viewed as a special case of social science whereby the objects under study do not exhibit self-consciousness. (Biological science perhaps falls in between the extremes.)

As human self-consciousness appears in the domain under scientific consideration, not only must it be dealt with in theoretical formulations, but the formulations themselves are inextricably bound up with the values and world views of the formulators. There still are empirical events which can be used (after agreement on their relevance, reliability, etc.) to test one formulation against another. But there is an increasingly hazy distinction between "science" and "ideology." These issues were merely touched upon rather than explored in the workshop.

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* Leonard Goodwin (Chair), Robert Knapp (Reporter), Daniel Alpert, Vaughn Blankenship, Clair Blong, Howard Davis, Kirsten Gronbjerg, Ian Mitroff, David Rose, Fred Rossini, David Schuelke, Carol Weiss, and Charles Wolf.

It did become apparent, that if scientific formulations have value components, then, surely the definition and policies with respect to social problems have value components. If scientists are to relate themselves to policy issues, they must be concerned with values and ethics.

The discussion prompted one participant to question whether social science has anything to say to a skilled practitioner. What could a political scientist, for example, have told Lyndon Johnson about running the United States Senate? Again, there was not time to explore this issue in depth. It was mentioned that science can include formulations regarding other systems and events that the politician might not be fully aware of, and that science can illuminate general issues that can be communicated to others, whereas politicians may see their experiences only in personalistic terms.

INSTITUTIONAL SCIENCE AND SOCIAL PROBLEMS

A pressing question raised, but not answered, was whether the education and professional experience of scientists tend to isolate them from the social problems they study and thereby to undermine the relevance of their research. The institutions of science may filter out kinds of people who favor social involvement as against those who prefer abstractions. If so, the rewards and institutional settings of science are set apart from the problem-solving agencies of society.

Several voices were raised advocating a distinction between research for "personal use" of scientists, carried out because of its aesthetic pleasure or its reward from the scientific community, and research for "social use". The former should be supported by public funds on the same footing as support for the arts or a symphony orchestra. The common notion of a linear relation of science to society, in which research done for its own sake is picked up by technology to meet social needs, may well be a myth.

Society subsidizes the scientific profession because it presumably performs a valuable social service. But scientific activities are not closely and directly attached to such service. Activities of scientists are dictated by matters such as personal research style, prestige, power, institutional rewards, as well as public service. If the linear model is in fact inaccurate, then, the public subsidy provided science perhaps needs to take a different form if this enterprise is to more effectively help meet social problems.

The relation between assumptions underlying research and those underlying policy perhaps raised the most interest. There was general recognition that a great diversity of values exist within society and even among scientists, with models used in research reflecting values of the investigators. Because of difficulties in dealing with diverse values, there is a tendency to rule out issues of ethics and values in policy discussions and to try to conduct applied research in a "value free" environment. One needs to avoid this easy way out, if research is to be socially relevant. How does one deal with the issue of divergent values?

TOWARD AN "EXPERIMENTAL" APPROACH

One step is to try to involve groups affected by research in its initial formulation. That is, there is need to find some useful way for policy-makers, victims of a social problem, and other interested parties to participate, along with scientists from relevant disciplines, in choices of research focus and methods. Attempts in this direction have been beset by a variety of difficulties, and will continue to be so. Scientists, for example, may find themselves interacting with persons who question the legitimacy of their "expert" knowledge.

The assumption was shared by many in the group that bringing different parties together is valuable only when these persons are willing to expose their personal problem-understanding models and to confront differences in how they think. The need is not just to be multidisciplinary (not just to have multiple representation), but to integrate multiple values. It is also clear that different conceptual frameworks have powerful emotional dimensions. How, then, can a means be created for helping persons with diverse frameworks interact and still be task-oriented?

The interactive process itself becomes the first focus of research, although the ultimate goal remains the amelioration or at least improved understanding of a social problem. One form of "experiment" in this kind of research is the design and creation of settings in which productive interaction can occur, in which multiple values and world views can be made explicit, conflict among them expressed and managed, and some synthesis achieved. One participant mentioned that he had carried out work of this kind with corporate managers.

We recommend that the proper direction for further work is through experiments of the kind described above, in which researchers and users participate jointly in an effort that has an action orientation. Holistic formulations of social questions cannot be done in the abstract. Specific social problems must provide the focus. Organizationally, we recommend the holding of a design workshop aimed at starting work along these lines on some particular problem.

Such a workshop should be organized to reflect the values it professes, namely, provide fruitful interaction among scientists and others who are attempting to solve social problems. The opportunity for dialogue must be built into the institutional design. This workshop would focus on creating knowledge which can be used in problem-solving, and it would encourage the explicit expression of values in the research design that might emerge. Ultimately, a research center might be established for efforts of this kind. But a first effort should concentrate on achieving success in the more limited venture that the proposed design workshop represents.

H.

RECOMMENDATIONS FOR IMPROVING MOTIVATION AND REWARD STRUCTURES*

Donald Michael
James Taylor

INTRODUCTION

A preliminary agenda statement to the workshop group by the Chairperson presented the workshop problem and outlined the factors and circumstances related to motivations and rewards in performing public problem-oriented research. The statement covered: (1) the spectrum of individuals and groups involved the entire process of generating and using such research; (2) the sources of motivations and rewards for researchers and their organizations; (3) the kinds of rewards for researchers, for organizations, and for the social environment; and (4) the (desireable) consequences of responding to incentives. Michael's statement offered some specific recommendations that the workshop group might consider.

Michael observed that, as with all other aspects of adapting science to social needs, the problem of engendering effective motivations and rewards contains complexities within complexities: it is itself a systems problem. The outline is an attempt to delineate some factors which seem to influence motives and rewards for the problem-oriented researcher and the several enveloping and mutually influencing societal contexts that generate and respond to such motivations and rewards. The position that what is rewarding and motivating beyond mere physical survival is itself very much influenced by what society defines as worthy motivations and rewards for carrying it out is implicit in Michael's approach. Taking such a position emphasizes that much of what comes from inside a person had its origin outside, for example, from the forms in which one seeks approbation. Organizations are also guided by socially given definitions of what actions are rewardable and what constitutes a reward.

These comments and the statement served as a beginning, to indicate that the larger reward structures of society transcend those of the laboratory or university in their effect on scientists and technologists. In order to adapt science to social needs, it may be necessary to integrate this reward structure with the process of research itself -- the way research problems are defined and sponsored, and the way research projects are designed and organized.

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* Donald Michael (Chair), James Taylor (Reporter), Suzanne Brainard, Harold Chestnut, Nathan Caplan, Bernard Cohen, Gordon Enk, Paula Gordon, Edward Horn, Genevieve Knezo, Saleem Shah, Arthur Weiner, and Raymond Woodrow.

DISCUSSION

There being some ambiguity in the workshop title, the participants initially agreed to focus primarily upon the motivations and rewards likely to encourage scientists towards greater involvement in social problems research. On discussion, it was clear that different participants approached this task from divergent views of the social change process. The group agreed, therefore, to solicit individual and specific recommendations as a possible strategy for bypassing the conceptual dilemmas. This approach resulted in 20 recommendations which are listed at the end of this summary. But while the conceptual issues were bypassed in this way, they were not eliminated. In the course of discussion a number of clarifying comments were made and discussed, a number of arguments erupted, and some recommendations were modified in consequence. Among the issues discussed were the following:

Differential Selection Into Research

A system is self-selecting for certain kinds of individuals by its motivation and reward structures. Certain systems effectively select for individuals possessing character traits which respond favorably to its pattern of rewards and penalties. Changing these patterns may change the attraction of certain fields, and lead to the recruitment of people with other or more varied character traits.

The Effective Motivators

There may be a vast discrepancy between what the organization or individual thinks is a motivating factor and what actually works as such. It is easy to overestimate the importance of tenure, raises, etc., as motivational forces. But perhaps more attention should be given to structural forces, such as the concept of "power," as viewed within a department or a university, or within an organization such as a professional society or a government research program.

The premise "different motivations for different individuals," though widely held, may be subject to examination. There may be extensive "common" motivators applicable to science and social problems research.

The Necessity for Choice

In order to motivate scientists and other professionals to work on social problems, there is a need to identify areas where research really can make a difference, and to specify the contributions scientists can be expected to provide along with other groups in the society. It may be useful to establish some sort of reward system which would acknowledge those cases where scientists have indeed "made a difference" in helping provide for the common welfare through scientific research. This sort of process, moreover, might also provide a disincentive by acknowledging some problem areas where more or better research cannot help with the solution. Upon reflection, this might not be a bad thing, if it candidly acknowledges the limitation of science in areas which require public decisions.

"Interdisciplinary" or "Policy-Responsive" Research?

Throughout the discussion, interdisciplinary research was seen as a distinct area that needs improvement, and that research responsive to social needs (policy-responsive research) is another distinct area. It is presumed that there is a linkage between the two, but this connection was not specifically articulated within the workshop or, indeed, within the conference. Yet there existed a recognition that to improve interdisciplinary research by itself may not make science more responsive to social needs in the short term. Rather, this effort was viewed as laying the groundwork for that responsive process.

RECOMMENDATIONS

From this discussion came 20 recommendations, as listed below:

Research Needs

1. More research is needed on how to get research utilized. Many pieces of applied research seem not to fit organizational needs; many research findings remain unutilized. We need to clarify the variables which lead to optimal utilization.

2. More research is needed on what research is needed. Given limited resources, some priority setting is necessary. It is not obvious that the current research investment accords with current and pressing human needs.

3. More research is needed on the kind of organizational forms which can sustain or facilitate systemic, interdisciplinary research in the public interest. Given the pull towards disciplinary specialization within current institutions, and among researchers, it has proven difficult to maintain interdisciplinary forms of research. Some comparative examination of institutions which have succeeded, and institutions which have failed, might prove helpful in the design of new organizational forms to this end.

Awards and Motivation

4. National awards might be offered for outstanding examples of problem-oriented research.

5. "Public advocacy" actions of scientists should be rewarded and protected. A number of scientists have gotten into serious trouble with their organizations when they testified in the public interest and against the policies of their employers. AAAS might be able to provide scientists with the same protections and sanctions for such activity as are now provided for faculty by the AAUP.

6. Other motivations besides "objective" rewards and punishments should be inculcated or appealed to. Humans are not simply reactive: they are also guided by internal goals of a more transcendent kind, a desire to promote the well-being of mankind, for example. These motives might be strengthened by example and training, and serve as the basis for commitment to research focused on social needs.

Special Resource Development

7. Special resources should be provided to organizations wishing to mount interdisciplinary research activities. Such efforts are expensive; and the start-up costs are considerable, since about 50 percent of the initial project time needs to go to resolving communication problems among the participants.

   Thus the costs of interdisciplinary research are greater than the costs of discipline-oriented research, especially in the early stages. and special resources are needed. Such resources might take the form of an institutional allowance, and/or a waiver of cost sharing.

8. Special training should be given to scientists at the graduate or undergraduate level to facilitate their skills in interdisciplinary communication.

9. Special resources and support should be given to scientists interested in moving into new areas outside their own disciplines.

10. Since interdisciplinary research directed towards social needs requires change in organizational structure, special resources should be provided to interested organizations to aid them in this organizational development effort.

Roles for AAAS and the Professional Societies

11. AAAS should do everything within its power to legitimate research in the public interest.

12. Deliberate efforts should be made by AAAS to disseminate public interest research findings of special significance and scientific merit.

13. AAAS should facilitate the development of research oriented towards social problems through the sponsorship of special conferences on particular issues.

14. Pressure should be put on professional organizations which currently deprecate applied research.

Government Actions

15. High status advisory groups -- "Council of Social Advisors" or "Council for Social Analysis" -- should be instituted in the Executive Branch of government. comparable to the present "Council of Economic Advisers." Apart from its usefulness, such a council would help legitimate applied, policy-oriented research.

16. A national clearing house (perhaps affiliated with NSF-RANN) should be set up to disseminate research relevant to social policy.

Educational Changes

17. Opportunities for participation in problem-oriented research (perhaps through internship programs) should be incorporated in secondary and higher education.

18. An awareness of the potentials of research oriented to social needs should be more widely disseminated among lay users, and throughout the scientific community among professionals.

19. Issues of social responsiblity should be introduced early in socialization, perhaps at the primary school level.

20. A perceived imbalance in major universities between educational, service, and research functions should be redressed.

I.

RECOMMENDATIONS FOR CREATING EFFECTIVE MANAGEMENT STYLES FOR INTERDISCIPLINARY RESEARCH*

Daniel Horvitz
Norman Evans

This Workshop reviewed the discussion of Workshop C of the previous day, and focused upon research process stages to examine what features are particularly unique to interdisciplinary research management. The usual "management textbook approach" to research projects includes the following stages: (a) the forumulation of the problem definition by the research team; (b) the decision to undertake the research project; (c) the assessment of research capability within the organization; (d) the selection of the team, project director, and support staff; (e) the organization of the study plan; (f) negotiation of the contract; (g) start-up period; (h) work performance; (i) phase-out, reassignment of team and support staff; (j) sponsor review and comment; (k) evaluation of the results and team performance. In practice the sequence of steps can sometimes be much different: some steps are merged or completely omitted. Nevertheless, in the Workshop's attempt to define unique factors relating to the management of interdisciplinary research these steps provided a suitable focal point.

DISCUSSION

One example of a special aspect of interdisciplinary research is that the start-up period may absorb 50% or more of the project time and resources. This situation occurs because of the particular difficulties associated with the interaction of persons from different disciplinary backgrounds and the need for extensive user and researcher interaction during the early stages. Another special aspect or requirement is assessment of capability. Since the proposed or assumed capability may be fragmented in different departments or centers within an organization, a sizable effort often must be made to evaluate the completeness and quality of the resource capability before a proposal is offered on a particular project.

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* Daniel Horwitz (Chair), Norman Evans (Reporter), Kenneth Beasley, C. West Churchman, Robert Cutler, Phil Gustafson, Walter Hahn, Lowell Hattery, Kenneth Heathington, William Newell, Vernon Root.

There are various ways in which a decision might be made by management to develop an interdisciplinary approach to a particular research problem. If the sponsoring agency requires an interdisciplinary approach, then the decision to undertake the project is based on an assumption that this approach is mandated by the research contract. This is by far the greatest incentive for developing an interdisciplinary approach by the management of an organization involved in problem-oriented research. At the other extreme, the performing team itself might suggest to a research project sponsor that, based on their understanding of the problem definition, an interdisciplinary approach is required.

When a research team has expressed an interest in a problem area, what factors affect their decision to operate in an interdisciplinary mode or in a more traditional multidisciplinary approach?

- Obviously, if the management of the performing organization does not have a commitment to and an understanding of interdisciplinary approaches, then there is little likelihood that an interdisciplinary mode will develop at the project level. If such a mode does develop, it will be the exception and will most probably encounter numerous administrative difficulties within the organizational structure.

- It is equally obvious that even when management supports interdisciplinary approaches, such a mode is not appropriate for every research project. There must be an assessment of organizational capability before committing the team to a specific approach.

  This decision on approach also depends on the institutional environment as well as the organizational capability. The problem area under study must be accepted as "in the interest of" the performing organization. There are certain research areas some institutions will always avoid because of these environmental factors.

- As noted earlier, the start-up costs and difficulties of interdisciplinary research are often much greater than anticipated. Therefore, a decision to go with this kind of approach cannot depend solely upon technical competence in the various subject areas. There must be top level support and awareness of the time and interactions involved in creating an effective interdisciplinary research team.

As the Workshop chairperson noted in his agenda statement, management can be effective in bringing about productive interdisciplinary research provided a number of factors are present. First, management must be committed to the team approach in problem-oriented research. Leadership is essential here, and true leadership can follow only from commitment. Second, management must define research problems in interdisciplinary terms. There is a strong tendency for each person to view problems in terms of his own discipline, to be unaware of those facets which require the knowledge and expertise of other disciplines. We also are often unaware of significant points of interaction between the disciplines relevant to a problem. It is insufficient, for example, for a biologist to discuss the life cycle consequences of a polluting environment to a particular commercially marketed fish species, without also having those consequences translated into economic terms.

Third, management must develop compelling motives for using a team approach and achieve recognition of those motives by each and every prospective member of the research team. In a sense integration of the disciplines can only occur after a desire to team up has developed. Management must provide either a set of logical reasons which will serve to motivate the needed interdisciplinary approach or else provide an appropriate reward system to accomplish this goal. If individual publications remain the only basis for promotion and remuneration, there remains but little incentive to work cooperatively on team research projects.

Interdisciplinary research may be enhanced by creating appropriate settings for such research and by careful selection of staff. For example, research units or centers which are deliberately interdisciplinary in character can be created. Clearly, the director and senior staff of such a unit or center must be committed to the team approach. Just as important, staffing should be with people who are not firmly set in a disciplinary track. These people are usually younger people, although the workshop group felt that senior scientists often perform well.

While a particular institution might deliberately set out to develop a suitable interdisciplinary research capability through changes in its organizational, management, and reward structure (and clearly changes are essential), it is doubtful that the capability would be adequately utilized unless at the same time there was recognition of the need to support such an effort on the part of funding agencies. Government programs and foundations that support policy-oriented research can provide the necessary impetus by explicitly recognizing that many public problem-oriented research projects require research groups containing a number of appropriate disciplines and operating in a fully interactive mode.

RECOMMENDATIONS

Based on these observations, the Workshop group developed several recommendations.

- Obviously, the research organization should provide guidelines and/or training for interdisciplinary research teams, preferably at the proposal drafting stage (especially for inexperienced teams).

- Interdisciplinary research management is not an established art nor widely understood. Therefore further R and D in research management is needed.

- There should be greater efforts by sponsoring agencies to help research organizations overcome some of the barriers mentioned in the discussion and get into the interdisciplinary research mode where useful and appropriate. Perhaps these efforts could be further implemented by the professional associations or the science community itself, by organizing seminars, guidelines, institutional consultation, and other informational forums.

- The AAAS should consider providing services (seminars, training, guidelines) to research organizations, designed to assist them in preparing to organize and manage interdisciplinary research.

- As virtually the only source of guidelines on research on the management of interdisciplinary efforts, the findings of the NSF Research Management Improvement (RMI) Program should be assembled and distributed widely.

- Greater user involvement in research monitoring and follow-up should be encouraged by sponsoring agencies, in order to promote application of the research results.

J.

RECOMMENDATIONS FOR NEW ORGANIZATIONAL DESIGNS: ADAPTING OLD INSTITUTIONS TO NEW FUNCTIONS*

Thomas K. Glennan, Jr.
Gerald Gordon

The Chairperson stated that the major premise of this conference is that more problem-centered research institutions are required if science and technology are to make a larger contribution to solving national and world problems. He observed that while it may seem easier to create new organizations to deal with new public perceptions of problems, prudence suggests exploring the possibility of adapting older organizations to our newer purposes. These institutions contain the bulk of our talent, with forms of security and personal comfort that would make it difficult to attract the best people away. Many services provided by an administrative organization are difficult to initiate; benefits accrue from building on existing administrative capacities. Proceeding incrementally towards new organizational forms may reduce the difficulties experienced in creating these new forms. (Of course, proceeding incrementally may also mean that you never get there.) Finally, many of these organizations, universities, national laboratories, not-for-profits or contract-research houses are genuinely anxious to make better contributions to understanding and alleviating social problems.

The question, then, is what advice can be given to those who would create such institutions?

CHARACTERISTICS OF PROBLEM-CENTERED RESEARCH INSTITUTIONS

Annecdotal experiences, some research, and some speculation suggest a set of characteristics that these institutions should possess. Among these are

1. Interdisciplinarity. Understanding and solving problems requires people with a variety of disciplinary viewpoints. It requires an organizational structure capable of (a)

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* Thomas Glennan (Chair), Gerald Gordon (Reporter), Sherry Arnstein, Kan Chen, Ronald Corwin, Donald Gerwin, Arie Lewin, John McKinney, Betty Pickett, Richard Rettig, Allen Rosenstein, and Leslie Rugg.

assembling adequate numbers of individuals possessing the necessary competences in the requisite depth and (b) rewarding these individuals for their contributions to understanding and solving problems and to the useful functioning of the organization itself.

2. Participation of ultimate users of the research and development in formulating the program of R & D. The definition of the problems to be worked on and the style of that work cannot be solely at the discretion of the research community. Not only are researchers lacking a sufficiently broad view to conceptualize the problem by themselves and traditional disciplinary paradigms inadequate to provide guidance in problem of definition, but also, involvement of the (public policy) user enhances the likelihood that the research will in fact be used.

3. A concern for the utilization of research. Policy research organizations must be concerned with how that research will be used as well as the structure of research itself. This concern affects the attention given to the form of presentation and the technical assistance provided to aid in its application. At a more subtle level, the concern affects the variables emphasized, the analytical techniques used, and the time horizons chosen for the research.

4. A recognition of the political aspects of the problem. Solutions to public problems will occur in the political arena, not in the halls of problem-centered research institutions. If research and development is to contribute to these solutions, it must not only recognize the value issues within a problem area, but also illuminate rather than hide them. Researchers must gain credibility with the various problem-area stakeholders so that the research work will be taken for what it is worth rather than for what interests it is perceived to serve.

5. Managerial quality.

6. Sensitivity to the different values involved in problem-oriented research.

The Workshop discussion stressed the centrality of organizational design in applying research to social problems. Relatively few people are sensitive to the need for designing a capability for organizational responses to changing environmental conditions. A feedback mechanism is a necessary part of this overall design, in order to know whether the mechanism is working as intended. Yet the funding sources for research appear to have a persistent bias resulting in neglect of the impact of organizational design on problem-oriented research. Fragmented structures cannot generate holistic kinds of research in the absence of an integrating design.

DESIGN OR MANAGEMENT

Even accepting the validity of that observation, the question remains: How does one design a problem-oriented organization sensitive to user demands, and create an environment sensitive to changes in the users' needs? Some organizations, such as RAND, have been able to change their user orientation over a period of time. They developed new capabilities in response to an altered environment. But it is problematic whether this kind of sensitivity can be built into an organization. A certain managerial attitude is required as well to achieve this sensitivity in the organization, and may in fact be the dominant factor.

In designing user involvement, the "simple" ideas about user input don't work for problem-oriented research. There is a complex interaction between the researcher and user which affects both the problem definition and research work. The product which emerges is that of true collaboration and it is often difficult to determine where the ideas actually came from. In this case there are not practical distinctions between "knowledge producers" and "knowledge users": there is an interactive flow connecting and blending the two identities. The producer-user terminology is a product of the hardware technology model of the research-utilization process, which includes the now questioned delivery system approach.

Problem-oriented research requires a different kind of terminology and a more interactive model. Changing our concept of this process must occur as part of the "adapting science" framework. There have been some experiments -- through oversight committees, liaison networks, etc. -- in creating research organizations within user groups responsive to their particular needs. Many of these research teams still suffer basic weaknesses in design, and are thus unable to realize their full impact on the problems of their sponsors. Users and researchers need to be fully integrated in the problem definition phase as well as the evaluation phase. A research "product" cannot simply be handed to an intended user.

RECOMMENDATIONS

Several Workshop participants noted the need for certain kinds of organizations which might be more effective in applying science to problem-solving activities. One such organization might be an Institute for Applied Research, possibly affiliated with a university; another might be a National Foundation for the Professions, which would support and encourage the professions in the same ways that NSF has supported the sciences.

The Workshop recommended

- Efforts be undertaken to increase the appreciation of the role of the professions in problem-solving activities. Perhaps a commission should be created to study this and make recommendations for change, or perhaps the AAAS should try to heighten the awareness, within its membership, of the potential contributions of the professions;

- Further airing, examination, and discussion of the recommendations of the "Simon Report" (Social and Behavioral Science Programs in the National Science Foundation, NAS, Washington, D. C., 1976.)

- Greater efforts must be made in future problem-oriented work to integrate users and researchers in the problem definition and evaluation phases. (The discussion recognized that the users must have a stake in the output if their input is to be more than casual.)

K.

AFFECTING THE ENVIRONMENT FOR PROBLEM-ORIENTED RESEARCH: GOVERNMENT FUNDING, AGENCY ATTITUDES, PUBLIC MARKETS*

F. Tomlinson Sparrow
Edward Poziomek

The themes of the Workshop were addressed through a series of questions posed by the chairperson. These questions and some indications of the agreed-upon responses are discussed below. Throughout the discussion, the focus shifted back and forth between the sites for and accomplishment of problem-oriented research and the federal sponsors and shapers of that research.

USER-ORIENTED RESEARCH

1. How Should A User-Oriented Research Program Be Structured In An Adversary Climate?

- There are many different examples of individual cases where scientists have been involved in providing information and advice to policy-makers on a topic of highly controversial or political value. These examples include:

  NSF funding of a study by the American Physical Society on light water reactors;

  NAS study on radioactive fallout in the 50's and the 60's;

  Various "virgin approaches" which involve NSF or other disinterested agency funding of a research team with no established reputation in the topic under study (an example here would be the Oklahoma team study, headed by Don Kash, of the Outer Continental Shelf);

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* F. Tomlinson Sparrow (Chair), Edward Poziomek (Reporter), Richard Bolt, Robert Cutler, Burton Dean, Harvey Dixon, Don Kash, Henry Lambright, Ann Macaluso, Ann Maney, Ernest Powers, William Queen, Robert Rich, Richard Scribner, Joel Snow, and Christopher Wright.

> Other approaches involve "the balanced portfolio," the "arbitrator" and the "mediator" roles which science has been called upon to execute in various situations;
>
> Suggested new approaches include the proposed so-called "Science Court";

The group did not wish to endorse any one particular approach, but rather emphasized that there are various ways of using science to resolve, or in some cases, to avoid conflict.

There are some specific criteria which should be met in structuring such a research program:

> The approach should seek to incorporate the highest technical competence available;
>
> The investigators should represent a "disinterested party" and not have any overt vested interest in the research outcome;
>
> All participants -- including the identified users and policy-makers -- should be involved in the study from the beginning.

Within a climate of adversary positions, it was agreed that individual circumstances would determine which approach should be selected. There were several warnings, however, regarding the dangers of research involvement in a situation which has evolved into a "zero sum game," where additional facts or information cannot affect the positions already taken. If the sides are already drawn, the decision process becomes a matter of political choice and the scientist has to be careful not to be "captured" by one side.

There is some concern as to whether "disinterestedness" can be built into a research team in terms of the professional biases resulting from the separate disciplines involved in the study. It was suggested that there are some structural counterpoints which can be established to counteract these biases, but these structured interactions should not restrict the information flow between team members.

2. How Should Responsiveness To Changing User Needs Be Balanced Against The Need For Maintaining Continuity Of Research Topics?

- There are some examples of a middle ground between "quick fix" research awards and long-term institutional support. Some government program managers are able to support an area of research and yet maintain a focus on changing agency needs by awarding funds over a period of time for individual projects in one research program. The program is thus "on call" so to speak, but the researchers are able to adapt to changing user needs with the development of each new research award. The trade-off to be made then is responsiveness versus continuity of funding.

  Although the preferred route for sponsorship of this kind of research is through a nonuser mechanism, there are some examples of successful programs, such as the Institute for Research on Poverty in Wisconsin, which might be viewed as an "agency captive" research center. However, it seems that to insure success of open ended research funding there must be some structured form of oversight and liaison mechanism built into the research program award.

  Within the discussion it was agreed that a definitive funding strategy for program support through nonpromotional and nonregulatory agencies would be a necessary step in developing an applied research capability within a given subject area. However, this program support should be coupled with certain provisos:

  - The program support awards should also include some project by project funding;
  - Program support should be seen as one part of an overall package of the problem-solving effort;
  - The research group should be required to interact with potential users;
  - Program awards should be given to several research centers (parallel funding) in order to stimulate intellectual competition. The risk of duplication must be balanced against the benefits to research quality resulting from such competition.

  There was also some attention directed toward the problem of turnover in research personnel, especially project leaders, as a disruption in the quality of research resulting from program-support awards. The question of whether program awards should be given to the project directors (and thus transferrable between institutions) was also discussed but not resolved.

With regard to the "intellectual competition" stimulus of duplicate program awards, one participant noted that the original mission of RANN had included an assumption that RANN would develop a "competitive alternative" in some problem areas where mission agencies appeared to be administering a sluggish research program. With the evolution of RANN, however, this notion of a "competitive alternative" seems to have disappeared.

IDENTIFYING NEW TOPICS

3. How Should New Topics Be Identified For A Problem- Or User-Oriented Research Group When They Are Not Associated With A Mission Agency?

- There was agreement within the Workshop that program planning by all research sponsors -- independent of whether they are within a mission agency -- has to anticipate the future. Anticipatory information is needed in order to develop an agenda of research targets and program goals, particularly by an agency such as RANN, which seeks to develop research based on national needs.

  At the same time that the recognition of the need for this agenda-setting capability is developing, there is awareness of the extreme difficulty in justifying this kind of research to the Congress. Anticipatory research needs to be legitimized; it should be made more visible, particularly within the non-mission-oriented research programs. Should a group such as RANN assist other agencies in these agenda-setting efforts? If RANN does not get involved in this activity, another agency (perhaps less qualified) will probably assume the responsibility. In fact, there is some indication that GAO is already assuming a larger role in the policy analysis area, and is thus diminishing RANN's dominance in this area.

  The workshop agreed upon the following recommendation:

  -- RANN should fund a study reviewing the processes of agenda setting, and examine the successes and failures of past agenda-setting efforts. It is hoped that from such a review, greater legitimacy would be given to anticipatory research efforts, including futures research.

ORGANIZATION

4. How Should A Problem/User-Oriented Research Group Be Organized?

- The discussion focused on three kinds of research sponsoring organizations: a problem-based organization (such as RANN); a discipline-based organization (as recommended by the NAS Simon Report); and a free-standing agency (such as NBS). It was agreed that a problem-oriented research group needs to be a free-standing agency, outside of an NSF which is dominated by the academic, pure science, basic science ethic. This group needs to be immersed in the research problems themselves. Such an organization should be a joint venture between the executive and legislative branches. Such a research group should have a mix of in-house and extramural research activities, and it should be of limited duration in order to avoid provincialism and obsolescence.

RESEARCH INTO ACTION

5. How Can The Prospect Of Translating Validated User-Oriented Research Results Into Changes In Action Programs Best Be Realized?

- The chairperson's agenda statement cited the California Innovation Group approach as one example. The Workshop recommended that

  -- Federal agencies should be encouraged to fund more research on knowledge utilization processes which accompany the production and application of knowledge. There is a need for experimentation with control groups in this area in order to obtain data about the processes of knowledge utilization.

" ... I ... suggest a future for those of us who believe that the world of (scientific) knowledge should come much closer to the world of practical needs and goals. The world of knowledge desperately needs to have understanding of how other worlds understand and behave."

C. West Churchman

VI

OBSERVATIONS, DILEMMAS, AND ACTIONS

" ... (at the) end of an interdisciplinary team project ... you don't file a final report ... the report may only be incidental to this process. Communication with the client ... may involve a large variety of interaction communications in a variety of media with a large set of people before anything really begins to happen."

Walter Hahn

OBSERVATIONS ON INTERDISCIPLINARITY: ITS NEED, MANAGEMENT, AND UTILIZATION

Walter A. Hahn
Senior Specialist in Science and Technology
Congressional Research Service,* Library of Congress

I was asked to play a role in terms of coming without any prepared paper, so that at least one person had the full-time task of listening instead of talking. I was to listen and see how, at least from one point of view, our topic evolved and to comment on some of the things that I heard. While constrained from being deliberately positive or deliberately negative I was encouraged to be, on behalf of all of us attending, self-critical.

Like the rest of you I didn't leave any of my biases at home. I brought them all along and you'll hear those come out in my reactions (along with everyone else's)! Some of them stem from the fact that I'm both a doer and a user of this interdisciplinary team research -- policy research -- problem research, that we've been talking about. My bias is client-oriented. I work for the Congress. I have 535 clients and they have me as well as a large staff of other people to respond to their requests for analyses and information. I don't give any research grants nor do I receive any. Thus, there may be some differences in my attitudes about some of the subjects we've discussed.

Like many of you, I used to be a card-carrying reductionist. I was trained as a physicist and learned all of the good or bad habits we've referred to. Some people think I went wrong because I also did advanced work in the behavioral sciences. Then, I really tripped over my feet because I joined people like West Churchman in the operations research-management science business. Most recently I have been engaged in futures research for the Congress. In addition, I'd like to go on record as being the second optimist that has stood at this platform. I notice that the last one here got polite applause but nobody seemed to act upon what was said.

I will try to do three things in a very brief time. I am going to describe one project that I am sure everyone in this room will agree is holistic. I hope it will serve as an example that this can be done, whaever this "stuff" is we're talking about. I will also discuss one reductionist problem that no one in this room can avoid. And finally, I will make a few observations on some of that things that I heard.

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* These remarks are the responsibility of the speaker and do not reflect the views of the Congressional Research Service or the Library of Congress.

INTERDISCIPLINARY RESEARCH -- SOME CHARACTERISTICS

First, we really haven't described -- and I don't think I'll be able to do it either but I'll give you some of the flavor -- what is interdisciplinary team research. What are some of the characteristics that are more unique to that particular set of activities than other activities? That gives me a lot of latitude. We have used the words: holistic, systemic, bigger than usual but small enough to understand and many more. We've said that interdisciplinary research is problem or opportunity focused, implied a consensus that the boundary lines are fuzzy, and stated it is probably an open-ended game. I at least heard (or maybe it's because I like to hear it), that it is client-, decision-,action-,and problem-oriented.

Time is a major constraint. If we are going to aid a decision-maker, we must recognize that something has to happen at some finite time. Hence, we can not just pursue research endlessly as can be done in some other pursuits. There is a mixture of qualitative and quantitative factors. There are objective and subjective data involved. I think there is some feeling that one should include almost any kind of additional information that will help, but that generates, of course, the problem of information overload.

Interdisciplinary research involves more than practitioners. People with disciplines or multiple disciplines or people with processes, procedures, methodologies -- whether they are disciplines or not -- become involved. Also included are those in the power structure: the decision-maker, the client, whatever we wish to call him. I was happy to see the number of times others beside myself have identified the fact that the affected party -- or the public in general -- is also a part of this process. That message was not as loud from some of you as it was from others. But the participants in this process are more than those of us who have the fancy algorithms and proceed via accepted paradigms.

There are also a few characteristics of interdisciplinary research concerned with its management that have been brought out, and I will mention a few of them. Again, these are "more unique" to this particular category of research activities than to others. Interdisciplinary team research is directed, led or managed: somebody has to be in charge of the team if the message would seem to come through.

There is a very long interaction period; to get started, to get to know each other, to rub off our vocabularies, to build communications, to develop a sense of common purpose, and to take that first cooperative step as a group. Some have estimated that this may consume over half of the total project time. Call it problem definition -- sniffing, telling sea stories, playing Ping Pong, whatever -- it does consume a large amount of resources and time to get going and it is an essential part of the process.

Similarly, on the other end of an interdisciplinary team project a long time is spent "communicating." Don Kash has experienced this and so have some of the rest of us. You don't just file a final report. As a matter of fact, the report may be only incidental to this process. Communication with the client is not just one quick chart presentation in an afternoon. It may involve a trip to somewhere. It may involve a large variety of interaction communications in a variety of media with a large set of people before anything really begins to happen. And there is a delay time before something happens, before results are observable.

Interdisciplinary team research is an iterative process. We add and drop information throughout. For example, on the White House National Goals Staff we started one of these projects with ten people, expanded at one time until we had about 150 consultants with us, and in the interim two of the core ten people literally dropped out. The humanist could never communicate with the physicist, or vice versa. And one of the operations people found that on this project he just was not functional.

In this process we need an unusually high tolerance and a respect for each other, for each other's jargon, approaches, methods, ideas, for the wholeness of the thing, and that does not come easy.

There is strong pressure in performing this type of research to have what someone labeled "geocentric" facilities (we love big words). Anyhow, we all have to live together or at least work together in one facility. Doing this on a part-time basis and in distributed locations, makes it harder, to say the very least, if not impossible. In a moment I will give you an exception where that does not hold. We have a lot of communications with the outside world. To get information into the project and to get information out of it, we need a commitment by all parties that it is to be a cooperative effort. We have to give up something to do that.

A HOLISTIC PROJECT

Let me talk about that holistic project that I think none of us can deny as holistic. I had the privilege of attending the Club of Rome meeting two or three weeks ago in Philadelphia (their first American meeting). Incidentally, there is one member of the Club of Rome here and he will straighten out any mistakes I make and you can ask him questions about the project rather than me because he is the one involved in it.

The Club of Rome reviewed the last two projects it sponsored and publicly announced the third. With respect to the original *Limits of Growth* study by Meadows *et al.*, they essentially felt that what they did was to alert the world's citizens to the idea that we do indeed have a problem in that we may not be able to survive on the earth as a people. The message got across: the possibility of self-extinction by something other than the bomb seems perfectly clear to a lot of people. They don't all agree when or that it will happen, but many agree that it is a frightening possibility. They also communicated to many by the report (sometimes to J. Forrester's disgust) that our methodology was inadequate to study such huge problems as Global Problematique. One speaker claimed they made almost every economist in the world equally mad and that this was a silly way to go about it. That prompted, of course, the second study. But they also felt that the *Limits of Growth* helped us realize the need for cooperation among peoples, among nations, among religions. They didn't say disciplines, but the implication was there.

Hence the second study, the Mesarovic-Pestel effort, led to the report, *Mankind at the Turning Point*. At Philadelphia they reviewed it, indicating that, yes, here was one alternative -- one of some 27 projects that they had been engaged in provided an alternative way of thinking about these global problems. It was a combination of reductionism, of course, because it has ten regional models for the globe. But in addition it had a holistic connotation in the fact that the only thing you really wanted to do was keep all ten regions operating. If one had to subdivide to get or manipulate some information, fine, but it was the total picture that had meaning.

The Club also sponsored in parallel to the Mesarovic-Pestel approach, Jan Tinbergan's proposal for Restructuring the International Order (called RIO, of course). This project is being implemented in a wide variety of places; literally around the world. One of the things observed was very dramatic and here we may have some hope. It was the image one got watching, first, an Egyptian, then an Algerian, then an Iranian (and there were others) stand up and describe their computer models as part of RIO (their structure as part of one or more of the sectors). They had different languages, different cultures, to some degree different religions, and, incidentally different disciplines. They were operating, within one sector, joint models that could talk to each other on hot lines in real time. They were operating cooperatively across those tremendous barriers, far bigger than the ones we have been discussing here. And that gives me some hope that maybe the barriers we see just between us in this room can be broken down so that interdisciplinary team research can go on. These modelers are not just "fuzzy headed" guys from universities playing with computers. They were, in this case, representatives of the official planning agencies of the three countries interacting -- and there were others involved who did not speak at this meeting.

The presentations of the third study undertaken were announced publicly for the first time. This one sort of blows one's mind (even one presumptive enough to be on a National Goals Study). It is called "Goals for a Global Society." This project has been under way for almost a year Ervin Laszlo, formerly of SUNY, Geneseo, now with UNITAR, is the leader of this project. They are trying to add to the first study's alerting function and the second's mechanisms to deal with the world problematique: the human values options. Ervin has done some very interesting things including rounding up both statements of the explicit goals (the announced statements) of some 141 nations. He has them categorized similarily to the RIO groupings. He also has had scholars and observers (they sometimes differ), looking at the *de facto* goals of many of these same countries -- how do they *act* versus what hangs on the walls or what is put in the papers -- and then searching among these for consensus and alternatives that could be "global goals." They announced a statement of "inter-existence," a word I think we are going to hear a lot more of.

The message of all this is that we must think holistically, but they did not bother to use that word often. It was emphasized that we are going to have to willingly cooperate and we are going to have to do it *now*, or face the possibility of death. To see people of this many religions, this many political persuasions, this many language barriers, fat and thin, and all the other differences one can think of, cooperating and acting in such harmony was quite a dramatic sight. Yet one of the sobering notes was whether there was time to do all this -- and that was not answered. It gives me hope, however, that we in this room can cross some of our much smaller barriers within a university, or within a public or private bureaucracy. If this much can be done by so diversified a group, why can't we do it?

There was one very dramatic event that stood out in this meeting of the Club of Rome, and believe it or not, it was a political speech. This is sort of an aside, but I think it's a real life note for us to bear in mind. Vice President Rockefeller was the banquet speaker and the first line in his speech was like a glass of cold water in the face of almost every Club of Rome member and the rest of us there. He said that anyone who thinks about limiting growth is naive! He went on for about 35 minutes. He indicated that the U.S. would pursue its elitist best, that we are a world leader. We will continue to expand our energy resources. He almost sounded like Herman Kahn. I have heard that one doesn't see members of the Club of Rome speechless very often, but this time they were -- in seven languages.

I think the Vice President did a great service. Some people tried to say a speechwriter gave him an antigrowth speech and he didn't read it prior to delivery. Well, those of us who have worked with this man don't believe that is a real possibility. He told it like it is. He gave a political speech and said (he did not use these words, but the message was): How can any of us go out and run in an election and say to the country you have got to give up your air conditioner. You have to give up your big car. We are going to lower your standard of living. You are all going to be happy, but you are going to wear little padded suits and they are all going to look the same. There is no way"Joe 6-Pack"is going to give up that which he has worked so hard for, particularly if his father helped him through college after the depression.

The Vice President's message was eloquent and very real. What is going to convince the voter to change not only what he has, but perhaps his aspirations also? The question he asked was not dealt with in the other excellent speeches of Philadelphia, but it went home in everyone's mind.

AGREEMENTS AT THIS MEETING

Let us return to this meeting. I heard us agree on a number of things. One of the things on which there is a consensus is that reductionist and disciplinary research is insufficient for dealing with social problems and that a more integrated and interactive approach is necessary. We need an approach that will include both the hard and the soft knowledge, as some one put it. We agreed that if we organize and put several heads together, we may be able to get around some of the larger problems that one head, or most single heads cannot totally grasp.

Believe it or not we were in accord on some terminology. We agreed that multidisciplinary is not interdisciplinary and that one is better than the other for approaching social problems. There was a consensus that interdisciplinary research is a means not an end. I think there was agreement (however, a little weaker than some of the others), that some intermediate form of knowledge is needed between the objective, descriptive and disciplinary knowledge of the analytical-contemplative world and the action world of doers and deciders. Chris Wright's rather thoughtful model presentation earlier expands on this very well. Although everyone of us in the room would put different labels on it, I think this concept is one that we can all take home and use. This "something in-between" might be called "policy-analysis," but it is not the label that is important.

UNKNOWNS

There were some unknowns, or at least some partial unknowns identified during this meeting. Here is where we agreed we didn't know, if you will. We are not sure we know how to run, or fund, or even legitimatize social experiments and yet there seems to be a feeling that some of these ought to be tried.

We don't know how to justify a large continuing investment or expenditure today for tomorrow's or next week's payoffs. It just doesn't sell. We don't know how to train or sensitize future researchers or managers in interdisciplinary research. Yet, there are hopes. There are some programs that are doing some of this. One fear is that we will learn how to do it and then make it a discipline and blow it.

We asked ourselves the question, can the universities be made a viable place for the study for interdisciplinary research? We have asked ourselves, or at least a few of us have asked, what are the alternatives? But, we begged that question in these sessions. We didn't argue the issue at this meeting, but it may be an important one for a future agenda. We raised the question and then skipped any attempt at answering as to what is the role of professions. We have some fuzzy notion about the set of activities involving decision-makers, decision-processes,and decisions. It is a murky area and we didn't shed much light on it here.

In one of the groups' discussion it was suggested that there are at least three classes of decisions which are quite distinct. Our team research and policy-analysis discussions may be able to benefit from defining them:

1. The executive official makes only a few decisions. He has a program or organizational commitment. He has in-depth, detailed information on it yet he doesn't get to make very many major decisions.

2. A legislator on the other hand, at any level, makes a very large number of decisions. He may have to vote, four or ten or more times a day. He often has very superficial information. Often he doesn't have a commitment to any existing program, but rather to a constituency or to himself. These rapid and ill-structured decisions may be bigger, more critical or more lasting in impact than the singular ones that the executive has.

3. The third class of decisions, may not be polite to talk about. They are the personal decisions. They may be opportunistic ones where the substance and the constraints around it do not really matter -- only whether "I'm going to get something out of it or not." They are made at high

levels and they are made at low levels. They are made by legislators, executives, academics, bureaucrats, and people on the street. We sometimes forget that these decisions can have major impact, particularly in the aggregate.

We raised some questions, which probably could be put more accurately leading to areas in need of research. How do we get the power structure to listen? The example was forecast in the '50s and '60s of the energy crisis. We bring the power structure bad news. As analysts we talk about long-range events but the decision-maker has problems now. We make forecasts and they don't know how true these are and neither do we. We make recommendations and they say to us as researchers, "You don't have to meet a payroll," or "You don't have to vote on the floor of the Congress," etc. Mostly we can not seem to get the power stucture to listen to our pleas for money, to let us go do our thing the way we want to do it. We don't know how to create new institutional forms. In the physical sciences we have learned to set-up laboratories and R and D teams -- actually to "produce knowledge" -- almost on demand. We don't seem to be able to do anything similar in behavior sciences although some representatives of think-tanks challenged this.

QUESTION: AREAS FOR RESEARCH?

How did we invent the corporation, the not-for-profits, the interdisciplinary team? How do we change a university, a bureaucracy, or even ourselves? We failed to treat these issues and we also asked the question, can we really approach a situation holistically? Can we view a situation without perceiving and understanding each element and its relationship to others? Systems analysts tell us about synergism -- that systems have properties beyond the sum of the parts. Experience tells us that we can see things holistically.

I am old enough to have been trained in so-called aircraft recognition during World War II. We learned all the parts of the Japanese airplanes. Then they would show us whole airplanes. We would look at them for 10 or 15 seconds on a small screen and the instructor would gradually speed up. The idea was that when we saw one that looked Japanese we would shoot at it and when we saw one that didn't, we wouldn't because that was ours. We got so we could recognize hundreds of these darn things each in a tenth of a second. Just a blink. I can look out the window and tell that there is a house out there without seeing every shingle and every window. I wonder if intellectually, in a far more advanced fashion, there isn't something analogous to this. If there is it would be very helpful in these large, ill-structured situations we increasingly face.

We are worried about creating a new discipline and we are worried about a few other things. We have some fairly large issues. Shall we, who have the skills and vision, be change agents or just attempt to get somebody else to make the alterations? Refer to Ken Heathington's paper. Are we going to sit on our tenure and ask for money, fiddle with our algorithms and do a lot of other "academic" things, or are we going to get out and do something? It seems like it is always someone else's task to do something but some of us may be competent to do things if we would try. A few have said we are, but institutional constraints hold us back. Is style a big issue? Of course we need basic research in substance and method and the opportunity for unfettered exploration. But is there not a parallel need to put to use for society that which we already know?

A key problem: who (read: what discipline) shall be the leader of the interdisciplinary team research project?

I am very concerned about problem definition -- which problem shall we work on? As an example (slightly overstated) those of us in the technology assessment movement are very concerned with always working on the right problem, the top priority problem. As a result, we haven't worked on very many problems. We are still arguing which ones to attempt. Some of us have a perpetual search for the new paradigm or a methodology. Obviously, from some of the biases I mentioned, I feel there may be many methodologies and many paradigms that can be useful. We recognize that the policy-makers need help now, but we talk about generating knowledge later -- is this part of the same problem?

There was some discussion about average people in super organizations and super people in average organizations as each being the best way of proceeding. Maybe we can use both. And we argued endlessly about reward systems, particularly the rewards which come to us.

One last question is perhaps rhetorical. Is interdisciplinary team research time, institution, geographic, personality, and subject matter dependent?

SOME IMPRESSIONS

Finally, a few last impressions including the self critical role I was asked to play. I hear it is impolite and incorrect to apologize in a speech. But when I see wall to wall reductionist toes out there, I might as well apologize now rather than later. I guess I am a little shocked at the tone of the perception with which many in this room view our clients, our patrons, "them." the Congress, the bureaucracy, and so on. Some of the phrases used include -- "that policy crowd," "politicians" (always with a little twist), "Washington," "regulatory interference," "irrationality" (of decision-making), and dozens more. We appear to have a conceit that "they" are not as smart -- whoever "they" are -- and we are very clear that they are not us, of course. I wonder if we should not be more conscious of how we appear to others and challenge the basis for some of our statements and mannerisms.

I particularly like the phrase, but more the meaning, of Dave Rose's point: "When the house is burning down, don't stop to check the pH of the water." I think we all ought to write that down.

We also act as if we just invented "holism." Or worse, that West Churchman is just about to invent it. But holistic thinking has been around for a long time. We only have to go back to Dan Alpert's story about the agricultural work. Or we can look around us and see what a city council, or a school board or a legislature does everyday. I am also a little alarmed how much each of us from our sector thinks the other guy's area is all messed up and that we ought to change it. We don't seem prone to looking in mirrors.

There was much hand wringing in these sessions over the university reward system but I didn't hear one proposal on how to change it, or any alternatives to it, or even any way to seize power. Are you just going to sit there?

The Executive Branch and its contractors blame all that they feel restrictive on political control, artificial limits, and "interference" from politicians. And when the Executive Branch comes up on the Hill, the Congress wiggles its finger at the bureaucracy. Polls show that people don't trust big business, or little business, and any size government for that matter. And we all blame the Arabs. Many of us researchers are concerned that we can't deal with these huge social problems upon us and yet we are intensely concerned about the reward system. We ought to think about this. There is no free lunch. We are plagued by doubt. We lack agreement. We have poor institutional forms and low resources. We admit a lot of ignorance, and yet our tone is grossly arrogant. We all seem to have the smarts about something, but we are not doing or learning as much as we think we need to do or we think we can. What is the problem?

Let me end by saying again that I am an optimist. I think if people from many countries can wrestle with such huge problems as the Club of Rome is doing on the Global Problematique, we ought to be able to handle some of these much smaller ones that we have been talking about here. I think we must do some introspection and take some active steps if we are going to survive in our little bureaucracies whether they are public or private. I think this meeting has been an excellent start on this and I for one would like to commend all who attended and the AAAS and others who have put it on. I am particularly happy that it is one step towards somewhere. And I am glad of two other things. One, that Don Michael is going to tell us how to get out of this dismal picture I have painted labeling myself an optimist, and that Dick is going to tell us where we can go from here at the end of this session.

"It seems to me then, that we end up with a wonderful and profound irony. The myth has been that the function of applied science is to reduce our vulnerability. What it appears to have done instead is to require of us that we accept participation in vulnerability, at a level of intensity that has not been required of any of us at any other time."

Donald Michael

LOOSENING THE SYSTEM FOR RISK TAKING: CREATING A "CHARITABLE" ENVIRONMENT FOR RESPONSIBLE AND IMAGINATIVE APPROACHES TO PROBLEM-ORIENTED COLLABORATION

Donald Michael
Program Head
Center for Research on the Utilization
of Scientific Knowledge

Two introductory remarks: First, the comments I'm going to make pertain to the scientists, the technologists, and to all the rest of us who gain status and a sense of reality by being dependent on and related to the community of knowledge-producers who we call scientists -- whether they be disciplinary or interdisciplinary.

The second introductory remark is to acknowledge that for many of you what I'm about to emphasize may have little to do with your perception of reality. But in my reading of these last three days, one thing seems to be clear: we do not understand the nature of that reality or what that reality should be. What I'd like to suggest is that my remarks emphasize something which needs to be part of whatever reality it is that you live in, and operate in, and hope to influence.

THE TWO-EDGED SOCIETAL CIRCUMSTANCES

I'm supposed to talk about the sources of loosening up the system so that it will be charitable to the kinds of activities and changes we've talked about. Let me start by suggesting that there are a number of societal activities, societal circumstances, already under way, that we've alluded to and sometimes overlook, but which need to be taken advantage of in order to loosen up this large system of users and producers, and consumers and funders of scientific knowledge applied to the public interest.

Before describing a few of these circumstances, let me acknowledge that they are very two-edged. We can hack our way through a jungle with them, perhaps, or we can bleed to death if we're not careful. But they are there. They do offer potentials for loosening up the system.

Demand for Participation

One of them is the demand for participation. I put the emphasis on demand. It isn't only a matter of inviting in participants, the ultimate consumers; it's that those participants, those ultimate consumers, are insisting on being part of whatever is affecting their destiny. Now, this source of pressure is extremely important because it is a pressure to be interdisciplinary at the very least. That pressure is to be applied to universities and university departments who fund sources, and to the conscience of scientists and those others who support and use science. It's a pressure that we can use to further the use of science in the public interest.

Challenge to Legitimacy

The second circumstance out there which can loosen up the situation is the endemic challenge to the legitimacy of conventional organizations and conventional reasons for doing things. This challenge to legitimacy can be used to discredit the parochial professionalism that stands in the way of so much interdisciplinary research and so much acknowledgement of the limits of particular disciplines for dealing with the real world. Responding to that challenge of legitimacy, which I say is endemic, is a potential way to loosen up those organizations which currently restrict inventions and activities of the sort we've been hoping to undertake.

Part of that challenge to legitimacy includes reservations in many parts of the population, including the scientific community itself, about the sufficiency of scientific knowledge. That challenge to legitimacy, that reservation about scientific knowledge, clearly leaves an opening to bring in other kinds of knowledge -- the kinds West Churchman talked about -- whether it be philosophical, ethical, mystical, political, or poetical.

There are many ways of communicating concepts and feelings of a holistic sort besides using the printed word or the spoken equivalent of the printed word. There's poetry, there's multimedia, there are storytellers, and so on. We haven't begun to include these as part of a holistic approach. But given this growing reservation in the society about the sufficiency of science, there are great opportunities to enlarge the "interdisciplinary."

A third example of how the challenge to legitimacy offers us some "loosening up" resources are "guerilla bureaucrat activities." These are sub rosa networks of people in government who are more concerned with the problem than the agency they're involved with: who are, in many ways, the models of effective knowledge utilization these days. They're the ones that are passing it back and forth. If it won't work in their agency, it will be passed to somebody in some other agency where it will work. For us there are guerilla networks to be part of; there are leakages to be encouraged -- in the interest of enlarging the concepts of the problems to be dealt with and disseminating the knowledge related to those problems.

Last among the challenges to legitimacy I want to mention that provide leverage for loosening the system, is the rise of consumerism and the enlarged range of opportunities this movement offers for scientists and others to participate directly in facilitating consumer's interests and to participate indirectly by using the consumer movement as leverage for breaking open some of the resistant organizations, whether they be federal funding entities, congressional offices, or professional societies.

Changing Values

The third societal circumstance that can be used to loosen up the system is the many changing values, conflicting value systems, value differences, which are also expressed in the demands for participation and the questioning of legitimacy. These value changes and value questions provide scientists and others involved with them (and the organizations they're working in), with more ethical maneuvering space, experimenting space, than in simpler times when there was a fair amount of convergency around what values and ethics were the right ones. That ethical maneuvering space can be used cynically, or it can be used to find ways to be more holistic, more responsive, more responsible, as we've been seeking to do these past three days.

An example of the constructive uses are the young scientists and bureaucrats -- I hope it's clear by now that "bureaucrat" is not a deprecative term but a description of a specific function -- who are far more preoccupied with the value implications of what they're doing than has been so in the past. They see themselves not as the agents of some higher power, or some employer, or some profession. They want to know *why* what they are doing is valuable, not just how to solve the problem that's offered them to work on. Now that's an example, it seems to me, of ethical maneuvering space that's available for loosening up the system in any number of ways.

Growing Ecological View

A fourth useful circumstance lies in the growing ecological, environmental movement -- a systems approach, an ecumenism, a participative approach -- that "gestalt" or world view which contributes to furthering a holistic perspective. That is, holism is an idea in increasingly good currency, which arises from a number of sources, terminologies, and groups. By its spread and its shared view, this idea offers an opportunity to generate support through a community that provides leverage in its own right. There are routes of appeal that weren't there before. There are ways of linking and of bringing leverage to bear because certain ideas are more legitimate, hence, more talked about, hence, more reasonable, hence, providing more opportunities for responding to them. In addition to making them more legitimate these ideas also recruit a lot more people and a lot more clout for pressing on those reluctant organizations and persons who aren't responsive to this holistic perspective.

One is that crises and disasters provide the best occasions for changes in organizations -- fundamental changes. The work done on disaster behavior over many years in many cultures, suggests that those are the times when organizations are most susceptible to fundamental change in philosophy and structure. The other thing that those social and natural crises and disasters will emphasize is the enormity of our ignorance and the limits of our information and theoretical bases for dealing with these problems.

THE RECOGNITION OF THE INSUFFICIENCY OF SCIENCE

That, in turn, leads me to a second general category of opportunities for loosening up the system (the first being those ongoing activities in society). The second category has to do with the growing recognition among scientists, as well as observers and interpreters of society, that the insufficiency of scientific understanding of social processes and social need is appalling. For all intents and purposes science is in a pretty footless condition when it comes to dealing with the social issues we feel are important. Further recognition of that ignorance and inadequacy could undermine entrenched professional and departmental powers and aid in transforming both statuses and leverages in directions more compatible with what we're seeking to accomplish by the way of using science for social needs.

What do I mean when I say that we're in this appalling condition? I need not review that picture in detail. It is sufficient simply to remind you of four circumstances. One, there is simply no theoretical basis available for understanding turbulent social change and the social processes that go on within a changing society of the sort we're in now. Ill-structured problems of this sort present enormous theoretical issues, for the most part unsolved, even in the physical sciences. In the social sciences we simply don't know what we're talking about when we discuss social change in a turbulent world like this. There are many critics within the professions making these points in greater detail -- including first-rate economists writing about the insufficiencies and bankruptcy of economic theory for the kind of world we're in.

Two examples of what I'm talking about by way of theoretical inadequacy: one is the fact that we simply do not know how to predict changes in birthrates. We never have been able to do it; can't do it now. And when you think of all that implies about the insufficiency of our understanding of human beings, of groups, of social process, that's pretty revealing indeed. Another example: as Walt Hahn and some others of you pointed out, we do not know how problems become recognized as problems in the society. For example, by what process does something become noticeable, hence attended to? We speculate but we don't really know.

Second point on the insufficiency of our knowledge and theory base: we don't have enough longitudinal data to know where things have come from, much less where they are going to. We don't have these longitudinal data partly because we have not collected them and partly because we lack validated theory that would tell us what to collect.

And, third, our methodologies, which may be elegant indeed, are footless because the methodologies presuppose the theory which points to what ought to be attended to, collected and analyzed. Since we don't have these theories: our methods, elegant as they are, are impotent. Nevertheless, as Don Kash emphasized and as a number of others of us would, we seek and depend on those theories as if our life depended on them. Indeed, our psychological life does in many cases. We try to convince ourselves that we know through our methodologies, but in cold fact those methodologies hang out there with no theoretical underpinning.

Last point. We're only beginning to understand how to change organizations and we have even less understanding of what to change them into because we're not clear as to what ends are to be met by changing into some other form. All we sense is that they need to be adapted, there need to be learning systems, and we don't know much at all about how to do that or, given our ignorance, how to learn how to do that.

The social turbulence described in my first category of opportunities for change and the conceptual insufficiency I've just reviewed, say to me that, for science to contribute in the way that we hope it would, scientists and the system that maintains them must become holistic in a far deeper sense than merely by being interdisciplinary.

THE IMPLICATIONS OF HOLISM

I'd like to emphasize some of the implications in that assertion. They're implications that we have toyed with at the edge but, to my mind, they are far more central and in need of attention than we have given them here.

To be holistic required one to have both an intellectual and experiential sense of being imbedded in the whole. It isn't enough to *talk* about being holistic -- one has to have the *sense* experience of being imbedded. This means, among other things, that given the circumstances I've described within science and society, it will be necessary to live with and acknowledge very high levels of uncertainty; not only living with high levels of uncertainty but acknowledging them because if you don't acknowledge the uncertainty to others, as well as to yourself, then you're not experiencing in the whole -- the whole system that has this quality of uncertainty.

This means that we, as persons and as organizations, are going to have to risk much to learn what science can do in a turbulent world, given its ignorance. This means we're going to have to risk ourselves, our organizations, our images. We're going to have to risk changing our theories, our methods, our status, our image that we convey to others and the images we carry inside of ourselves. It means we're going to have to risk trusting each other and other organizations far more than we do. Otherwise, we can't engage, we can't be enough a part of something that we're trying to be holistic about.

If you distrust, you avoid, you separate yourself, your activities, and it becomes impossible to be holistic under those circumstances. This kind of trusting involves a lot more than learning the other person's jargon. It means we're going to have to risk embracing error. It means we're going to have to realize ahead of time that we're going to make mistakes, and design our activities to make the most of those mistakes when they're made in order to learn. Otherwise, we can't learn, we can't understand what the whole is about.

Organizations are going to have to expose their errors as a positive expression of competence to learn rather than hide them in fear. We're going to have to risk recognizing that we, as persons and as our organizations, make the myths that are social reality and that, in turn, we are created by these myths. In our social roles we are myths, we are whisps, we are nothing enduring. We have to recognize that, by being holistic, we are finite. To my mind, the only way one really gets a sense of how finite one really is, by very consciously being part of the whole.

COMPASSION AND VULNERABILITY

This brings me to the word "charity" in the title for these remarks. Another way to emphasize all the above is to recognize that we do desperately need deep charity, or better, compassion towards self and others. And by charity, I don't mean indulgence or indifference or sloppiness or a *laissez-faire* approach -- since we're all ignorant let's all each go our own way. By compassion and charity I mean that we all have to recognize, as organizations and as persons, the fact of our profound vulnerability. We are all fundamentally vulnerable in our roles, our functions, our hopes. If we're going to be holistic, if we're going to deal with and come to discover the holistic kind of knowledge, that requires becoming very different selves as well as different organizations. This is threatening, it's frightening, it's painful, but I think we can't avoid such experiences if we're really serious about being holistic, in order to try to deal with the kind of problems that we acknowledge can't be dealt with piecemeal.

Can this be done? Can we make those organizational changes and those personal changes, threatening as they are, exciting as they are I should also add? As West Churchman said the other night, the injunction to do so and the seeking to do so is probably the oldest persistent vision in human aspiration. Individuals clearly have done it, small groups have done it for a limited time. The question is can a world or society, can even a nation or a region do it? And, I really don't know, but I would like to suggest that, in contrast to past civilizations that failed, we have a few things that might be going for us.

One, as this meeting evidences, is a deliberate self-conscious will to experiment, to discover. Second is that many of the turbulent characteristics I talked about are associated with a mood of transformation for some important parts of the society. And, third, in contrast to other civilizations, we can make use of historical perspectives. We can deliberately draw on the experiences of other societies at other times and that perspective is one that other cultures and societies didn't have.

All we can do then is continue to try. To do so means we must move in the ways we've been asserting that we want to. We must do so with an appreciation of the possibilities for loosening the system. And we must recognize and accept the unavoidable pressures this will put on us as individuals, both as private individuals and in our organizational responsibilities as well.

It seems to me then, that we end up with a wonderful and profound irony. The myth has been that the function of applied science is to reduce our vulnerability. What it appears to have done instead is to require of us that we accept participation in vulnerability, at a level of intensity that has not been required of any of us at any other time. We have to participate in that vulnerability if we're to have any other chance at all of producing a better world. Living within that irony, that contradiction and its resolution is, to my mind, what being holistic is all about.

* * *

COMMENTS

In a turbulent, changing world like ours, the possibilities for transformation are enormous. I remind you of the three General Electric nuclear reactor technologists who quit in order to further reactor safety. There are a lot of things of that sort that can be done and are being done. The fact that one is an institution, in an organization, to my mind, doesn't relieve one of the responsibility and the potentiality of making changes within it or outside.

We were talking at breakfast this morning about the fact that structured roles in an organization do not remove the enormous impact of the person who fills a role. We all know that from experiences. Things happen in organizations or don't happen quite aside from structured role performance. They happen or don't happen because of the person who fills that role. I'm arguing that if what we're talking about really is necessary then, many more of us have to be prepared to act as transformers in the organizations we're involved with.

Organizations don't exist without the people in them. The people in them sustain the structures of the organizations -- are rewarded or punished by these structures -- but are also what change them -- the people inside the organizations and the people inside other organizations that press on their own organizations. There's just no avoiding the fact that people are there. In our kind of society organizations can't survive in a given form unless people in them accept them in that form.

As we begin to see now, with the rise of consumerism, participation, and all the rest, organizations can be transformed by people. At any given time people are clustered into structured activities, but it doesn't mean that they're incapable of changing them. There are a lot of people in this room who have been involved in one way or another in changing the organizations that they're in, or changing some other organization by virtue of their personal actions or inactions. But it's costly.

Let me add two more things about the crises example to properly represent its possibilities. Crisis and disaster are the occasions when organizations can change. But, they can also close down and become more entrenched. Part of the condition that seems to determine whether it goes one way or the other is whether the organization has people either in it or around it who understand that process and are prepared to use it productively. For example, people who have been trained for change or trained for being creative in crises instead of just closing down in fear.

So, it isn't just a matter of creating crises, it's a matter of creating people and structures that are available to make a productive crisis. We know a little about doing that, not nearly enough.

The ethical issues are crucial. All I'm saying is that there is a whole set of potential leverage points for change beyond those that we conventionally think of, that in this kind of multiple value world in which we work, take on more potential than they had in the past for loosening the system and creating a charitable environment for problem-oriented collaboration.

Remember, I emphasized at the beginning that what I would attend to is not the whole picture. I offer these remarks as a counterpoint and complement to other parts of the issues that have been explored intensively here and that Walt summarized so well. Don't read this as an either/or business. Rather see the part I've emphasized as a necessary but not of itself sufficient part of the whole.

"A deep concern ... is facing every institution in our society. Not only universities, not only government bureaucracies, but every institution in our society today is faced with a challenge to its legitimacy, faced with deep dilemmas of just the kind we are having to face up to."

Daniel Alpert

THE DILEMMAS FACING US

Daniel Alpert
Director
The Center for Advanced Study
University of Illinois at Urbana-Champaign
Urbana, Illinois 61801

I've been faced with some curious dilemmas during the course of this meeting. At most scientific meetings my mind is challenged; I have trouble handling the subtlety and breadth of the intellectual issues. In other words, my head hurts. At this conference I've had more trouble with my stomach. At times, I felt exhilarated and greatly stimulated; at other times, I felt very depressed. I was concerned about the relevance of some of the talks and felt they removed us from the issues we came here to discuss. This morning I feel quite elated; Walter Hahn has given a far better presentation of what I was hoping to say during the first half of my brief summary, and Don Michael has made a far more eloquent statement of what I was going to say in the second half; so I have very little to add.

I came to this conference because I was attracted by its title. In fact, I knew very little about the proposed agenda other than the title, "Adapting Science to Societal Needs." Unfortunately, as I listen to much of our discussion, I'd have sworn that I misread it the title, I thought, must have been "Adapting Society to Science's Needs." This reversal of emphasis is one of the dilemmas that concerns me.

In his remarks yesterday, Dick Bolt was saying that you really can't solve a problem unless you care about it, unless you really get steeped in the symptoms and the problem context. In his description of personal experience working on a naval problem, he learned that you can't take the statement of the problem from the admiral; you've got to say, "Let me on board. I'll go on a cruise with you and get a sense of both the problem and the environment in which it occurs." You've got to feel, sense, and see the context in which the problem arises.

In the context of social problems, it is equally true that to solve the problem we've got to care. In some of the discussion at this conference, it has seemed to me that we cared more about the problem-solver than the problem. Our discussions have fully demonstrated an awareness of the fact that the problems are interconnected, that societal problems are deeply interconnected. It became apparent during our discussion, however, that the "problem-solvers" were not interconnected. This was demonstrated in many ways.

In one workshop, for example, we were supposed to address conceptual issues underlying approaches to the problems facing us. And we couldn't reach underlying approaches to the problems facing us. And we couldn't reach consensus or even agreement to discuss further any concept except one: the possible creation of a new federal foundation to provide us with money. To my mind, we had better improve our conceptual grasp of the situation if we think this is a feasible next move.

Dave Rose opened our Workshop with a discussion of morality, and in our conceptual discussions he insisted, "Fellows, we've got to teach ethics." My repeated concern was that learning about ethics - learning about any value-laden issue - is not something that can readily be taught. Learning, like lovemaking, is not a spectator sport; it is a participatory sport. Above all, learning about ethics is a participatory activity. Values cannot be taught; they must be lived and emulated.

I think these dilemmas are not just characteristic of this conference or of this group of scientists. A deep concern (that causes one's stomach to hurt as well as one's head) is facing every institution in our society. Not only universities, not only government bureaucracies, but every institution in our society today is faced with a challenge to its legitimacy, faced with deep dilemmas of just the kind that we are having to face up to.

Despite the fact that I have been deeply frustrated by these dilemmas and contradictions during the course of this conference, I have had occasions for feeling very positive. At one point, Dick Scribner asked me to make some summary comments at this session. I was rather pessimistic about our progress and said, "I'd rather not; I feel pretty sad about what I've been experiencing at the conference." At the time, I did feel very sad. A few moments later, as I was walking out, someone who overheard the conversation said to me, "I'm really glad you said that; I'm glad you are not proposing to gloss over your real feelings about the proceedings thus far. I hope you will make some comments tomorrow." This positive response to candor, to stating my concerns in public, and to admitting an open expression of feeling was a tremendous boost to my morale. Paradoxically, I suddenly felt very good about saying how bad I had felt.

THE PROBLEM IS A TRUE DILEMMA

I believe that the explicit statement of an impasse is perhaps the most optimistic situation we can hope for when we really know that we are in trouble -- when we have finally admitted to ourselves that the problem represents a true dilemma, and that it's not something we can just fiddle with. When we have admitted to an impasse, that's the time when we can make a choice. For this reason, I consider it an optimistic statement to say that I'm aware of certain true impasses in addressing some of our deepest societal problems. I'm aware that they are pervasive, and, if we acknowledge their deep subtlety, I'm not disturbed by them.

There's an old aphorism that says, "When someone points a finger at a real problem, professors will proceed to study the finger." Well, that's not so bad. Indeed, I'm beginning to think that we might push it a little further; we should look not only at the finger but at the person who is pointing the finger. That is, we should look within ourselves to examine who we are and what we are capable of contributing. And that, of course, was one of Don Michael's statements.

I have been preoccupied with our centers of academic learning and their divorce from some of the critical problems of our times. I've sometimes been very discouraged, lost, and a stranger in my own institution. Sometimes this discouragement was due to my own arrogance. I was asking, "How do you get the power structure to listen?" We are part of that power structure: How do you get us to listen?

During the course of this conference, I have felt occasions of considerable hostility among different groups. A friend of mine, Bob Fuller, taught me something about hostility; he taught me that there is an interesting and paradoxical relationship between hostility and love. In order for the white and black communities to approach any kind of reasonable discourse, there was a need for blacks to express hostility. Before love for others is possible, there seems to be a need for the freeing, the liberating features of the love of self. This was necessary to the self-respect of the black community; it is also true for other ethnic groups; it's true for individuals. It may be that we must address the hostility that we sometimes feel about each other before we can express love. It may thus be hopeful to see this kind of hostility manifested on the part of the blacks, on the part of third world countries, on the part of social scientists struggling to talk to physical scientists. Without such a manifestation of hostility, there can't be a freeing of people. Without freedom, we can't have love.

Thus, I believe that admitting to the dilemmas and even the hostilities among the various groups struggling with our societal problems today is the first major step to resolving them.

So, really, I am an optimist, after all.

" ... we need an unusually high tolerance and respect for each other, for each other's jargon, approaches, methods, ideas, for the wholeness of the thing, and that does not come easy."

Walter Hahn

ACTION ALTERNATIVES FOR AAAS

C. West Churchman

School of Business Administration
University of California, Berkeley

Rather than summarize the conference, I prefer to suggest a future for those of us who believe that the world of knowledge should come much closer to the world of practical needs and goals. The world of knowledge desperately needs to have understanding of how other worlds understand and behave.

Specifically, if we assume that workshops such as this one are ways of understanding then I suggest interaction between the "science world" and these other worlds, via such workshops.

1. The world of politics which is based on a universal human desire to be public, to matter, help, lead, follow, share, and care for others.

   Specific Suggestion:

   A AAAS workshop made up of interdisciplinary scientists, law experts (jurisprudes, judges, legislators, lawyers) and just plain average citizens.

2. The world of morality based on universal human need for equity.

   Specific Suggestion:

   A AAAS workshop made up of interdisciplinary scientists and the moral community: humanists, ethicists, feminists, worldists, poor people, and perhaps a scattering of economists, psychoanalysts, and so forth.

3. The world of religion based on a universal human need to be in touch with some ultimate force of the universe.

   Specific Suggestion:

   A AAAS workshop made up of interdisciplinary scientists and the religious community in the broadest sense, not only representatives of organized religions but also poets, novelists, painters, misfits, worshipers, mystics, and atheists.

4. The world of aesthetics based on a universal human need to be in touch with nature, our nature and all of nature.

   Specific Suggestion:

   > A AAAS workshop made up of interdisciplinary scientists and the aesthetic community, dramatists, painters, hobbyists, optimists, and pessimists.

All of the above worlds, I think, deal more in the spiritual than they do in the intellectual. Perhaps St. Paul had the best way of summarizing the spirit in each of these worlds: "We are everyone members one of another." (Romans:12)

THE NEXT STEPS

Richard A. Scribner
American Association for the Advancement of Science

I will not attempt to summarize all that has taken place in the last three days either. Instead I will review briefly some thoughts evoked by three questions: (I) Where did we start? (II) What happened here? (Or some of my impressions about what happened here), and (III) Where is it all going?

WHERE DID WE START?

In one sense, we all started with the Introduction, the Keynote Address,and the other material in the pre-conference document. In another more appropriate sense, most of us have been wrestling with at least some of the problems addressed by this conference for five, ten, or even more years. Despite the large amount of common interest and background (too much so, some of you have told me), we are just learning to understand each other when we talk about social problems and the contributions scientists and science institutions can make. The discussions, particularly those in the workshops, demonstrate this fact. However, we did formulate recommendations for change and improvement -- and maybe some of us believe we can find ways of implementing some of these recommendations. It is clear that we have not reduced the number of questions to be addressed regarding adapting science to social needs. Hopefully we sharpened some of the questions and pointed out some new directions which may lead to useful answers.

WHERE HAVE WE BEEN?

Some of the issues were joined the first evening after West Churchman's Keynote address. The Conference has had its share of paradoxes. The first plenary session and this last one make me wonder whether this conference was sponsored by the American Association for the Advancement of Science or by the World Council of Churches.

I am most grateful to all the people who contributed so much to this event, and thankful to Ken Heathington, Jim Taylor, and a few others who showed their good humor and helped us all to laugh. Without their wisdom the weight of our subject might have kept us altogether too sober and dull.

One way to recall the ground we have covered here is to note some of the phrases marking new or emphatic ideas. In looking at the nature of social problems, how science and technology may be used better in helping to solve these problems, and what are some sources of difficulty in seeking more effective use, people said the following:

> We need systems approaches and holistic thinking.
>
> Can you use holistic thinking to solve real-time problems?
>
> When the house is burning down, don't stop to check the pH of the water.
>
> The academic reward structure encourages fragmentation.
>
> Interdisciplinary research (IDR) is not accepted as a legitimate undertaking in academia.
>
> Interdisciplinary centers must have equality with university departments, if they are to survive in university politics; program support is necessary.
>
> Perhaps the university should not be involved in interdisciplinary research.
>
> Time deadlines for policy research are critical: one cannot always have a complete product.
>
> Setting policy research priorities requires user input, if the results are to be used.

Among the suggestions and recommendations for change, I heard

> We must learn to formulate the holistic research question.
>
> We need (to be more tolerant of) "intermediate" kinds of knowledge.
>
> In the absence of paradigms for applying interprofessional knowledge to public problems, we must nevertheless try by substituting interdisciplinary organization of people for theory.
>
> Interprofessional societies may offer a meeting ground for those who wish to work in the problem-oriented areas.
>
> Associations such as AAAS can do much more to make

> policy-oriented work a better, more respected, and more useful undertaking.
>
> We need greater emphasis on the professions in the area of public problem-solving.
>
> We need linkages between individuals -- not only institutions.

I am not certain what the implications of some of these phrases are, though I sensed that all of them have significant meaning in the context in which they were delivered.

WHERE IS IT ALL GOING?

I was told that some people were asking: What is my or the AAAS "hidden agenda"? The question makes me smile because we are very far on the other side from having a preplanned output agenda for the conference, and I am painfully aware of this. Since we are talking about taking risks and being vulnerable, I will admit that just where we will come out as a result of the conference is not clear to me.

A proceedings volume will be generated. I expect the results of this conference to have some impact on at least three aspects of AAAS: (1) The Board of Directors will read the proceedings with interest; (2) some of our recommendations may stimulate the Committee on Science and Public Policy and also become incorporated into AAAS public policy efforts such as the Annual AAAS analysis of issues in the federal R&D budget; and (3) the AAAS New Directions Committee, charged with preparing the Association for new roles and goals some five years ahead, may be quite interested in our thoughts here. West Churchman's suggestions for collaborative outreach to other professions and other areas of endeavor are particularly appropriate in this context.

Dissemination of the proceedings should place the results in the hands of many people interested in what we have done. We expect to prepare a summary which would be sent to others who might not receive or read a proceedings volume. And last, but certainly not least, several small group meetings may be held around some of the issues and recommendations of this conference. The participants will include some of this conference's participants and others, in government, academia, and elsewhere, who may be in a position to do something toward further clarifying the issues or taking steps to implement the recommended changes.

I have a few thoughts regarding possible topics and participants for these small group meetings:

Area of Concern:	Participants From:	Approach:
Adapting the university structure and reward system to better accommodate IDR	Government agencies Universities AAU, NCURA, AAAS, Sigma Xi	Structured approach and analysis; experimentation and evaluation; visibility
Enhancing the status of applied and policy-relevant research	Government sponsors, NSF Private foundations AAAS, IEEE, ORSA	Awards, visibility, publications, quality assurance
Linking knowledge centers to policy analysis and problem solving institutions	Facilitators of such linkages Government agencies, NSF, ERDA Universities Professional organizations	Understanding past efforts; fostering new experiments; supporting prototypes

We invite your further comments and involvement. Tell us what we did right here and what we did wrong. Send us appropriate information to augment this conference and its proceedings. If there is a strong minority view on any workshop summary, let us have it to include in the proceedings. If you have formed a useful alliance as a result of this conference or undertaken a project partly because of what happened here, let us know.

Finally, I emphasize that whether anything happens as a result of this conference and the work we all put into it, depends in large measure on what *we* (all of us, working within our own institutional and professional contexts) *do* when we leave here.

APPENDICES

APPENDIX A

PARTICIPANTS

ADAPTING SCIENCE TO SOCIAL NEEDS CONFERENCE
May 5-8, 1976
The Institute on Man and Science
Rensselaerville, New York 12147

ABT, CLARK C.
Abt Associates, Inc.
55 Wheeler Street
Cambridge, Mass. 02138
(617) 492-7100

ALPERT, DANIEL
Director, Center for Advanced Study
University of Illinois
912 W. Illinois Street
Urbana, Illinois 61801
(217) 333-6729

ARNSTEIN, SHERRY
Senior Service Fellow
National Center for Health Services Research
5600 Fishers Lane, Room 16-68
Rockville, Maryland 20852
(301) 443-5134

BEASLEY, KENNETH
Assistant to the President
Northern Illinois University
Lowden, 301
De Kalb, Illinois 60115
(815) 753-1122

BLANKENSHIP, L. VAUGHN
Head, Planning and Policy Analysis
National Science Foundation
1800 G Street, NW Room 425
Washington, D.C. 20550
(202) 632-5793

BLONG, CLAIR
Society for General Systems Research
Lisner Hall
2023 G Street, NW
Washington, D.C. 20006
(202) 343-7684

BOLT, RICHARD H.
Bolt, Beranek and Newman, Inc.
50 Moulton Street
Cambridge, Mass. 02138
(617) 491-1850

BRAINARD, SUZANNE
Minnesota Systems Research Inst.
2412 University Ave., SE
Minneapolis, Minn. 55414
(612) 331-8750

CAPLAN, NATHAN
Center for Research on the Utilization of Scientific Knowledge
Institute for Social Research
The University of Michigan
Ann Arbor, Michigan 48106
(313) 764-2554

CHEN, KAN
Goebel Professor
The University of Michigan
2517 East Engineering
Ann Arbor, Michigan 48104
(313) 769-1420

CHESTNUT, HAROLD
Corporate R & D, K-1
General Electric
P. O. Box 8, Room 3C38
Schenectady, New York 12301
(518) 385-8435

CHURCHMAN, C. WEST
Center for Research in Management Science
26 Barrows Hall
University of California
Berkeley, California 94720
(415) 642-3860

COHEN, BERNARD P.
Department of Sociology
Stanford University
Stanford, California 94305
(415) 497-3958

CORWIN, RONALD
Department of Sociology
Ohio State University
1775 S. College Road
Columbus, Ohio 43210
(614) 422-2308

COWARD, H. ROBERTS
University of Maryland
Center for Environmental and
Estuarine Studies
Inland Environmental Laboratory
College Park, Maryland 20742

DAVIS, HOWARD
Chief, Mental Health Services
Research Development Branch
National Institute of Mental Health
5600 Fishers Lane, Room 11-94
Rockville, Maryland 20852
(301) 443-6166

DEAN, BURTON V.
Dept. of Operations Research
Case Western Reserve University
University Circle
Cleveland, Ohio 44106
(216) 368-4140

DIXON, HARVEY
Stanford Research Institute
333 Ravenswood Avenue
Menlo Park, California 94025
(415) 326-6200

ENK, GORDON
Institute on Man and Science
Rensselaerville, New York 12147
(518) 797-3783

EVANS, NORMAN A.
Director, Environmental Resources
Center
Colorado State University
Fort Collins, Colorado 80521
(303) 491-5371

GERWIN, DONALD
School of Business Administration
University of Wisconsin-Milwaukee
Milwaukee, Wisconsin 53201
(414) 963-4337

GLENNAN, THOMAS K., JR.
Study Project on Social R&D
National Academy of Sciences
2101 Constitution Avenue, NW
Room JH-818
Washington, D.C. 20418
(202) 389-6768

GOODWIN, LEONARD
Department of Social Sciences and
Policy Studies
Worcester Polytechnic Institute
Worcester, Mass. 01609
(617) 753-1411

GORDON, GERALD
Center for Applied Social Science
Human Relations Laboratory
112 Cummington Street
Boston University
Boston, Mass. 02215
(617) 256-3048

GORDON, PAULA
4701 Willard Avenue
No. 1015
Chevy Chase, Maryland 20015
(301) 652-2839

GRONBJERG, KIRSTEN A.
Assistant Professor of Sociology
State University of New York
Stony Brook, New York 11790
(516) 246-5000

GUSTAFSON, PHIL
Director, Office of Environmental
Projects
Argonne National Laboratory
9700 S. Cass Avenue
Argonne, Illinois 60439
(312) 388-4517

HAHN, WALTER A.
Senior Specialist, Science and
Technology
Congressional Research Service
Library of Congress
Washington, D.C. 20540
(202) 426-6082

HATTERY, LOWELL
Center for Technology and
Administration
Hurst Hall, Room 206
The American University
Washington, D.C. 20016
(202) 686-2513

HEATHINGTON, KENNETH
Director, Transportation Center
The University of Tennessee
Knoxville, Tenn. 37916
(615) 974-5255

HORN, EDWARD G.
Chairman, Department of Biology
Russell Sage College
Troy, New York 12180
(518) 462-3869

HORVITZ, DANIEL
Research Triangle Institute
P. O. Box 12194
Research Triangle Park, N.C. 27709
(919) 549-8311

KASH, DON E.
Director, Science and Public Policy
Program
The University of Oklahoma
601 Elm Street, Room 432
Norman, Oklahoma 73069
(405) 325-2554

KNAPP, ROBERT
Member of the Faculty (Physics)
Evergreen State College
Olympia, Washington 98505
(206) 866-6663

KNEZO, GENEVIEVE
Science Policy Research Division
Congressional Research Service
Library of Congress
Washington, D.C. 20540
(202) 426-6030

LAMBRIGHT, HENRY
Assoc. Prof. of Political Science
Maxwell Graduate School
Syracuse University
Syracuse, New York
(315) 477-8406

LEARY, JOSEPH
Reactor Development and
Demonstration Division
Energy Research and Development
Administration F-309
Washington, D.C. 20545
(202) 353-4471

LEWIN, ARIE Y.
Graduate School of Business Adm.
Duke University
Durham, North Carolina 27706
(919) 684-4266

MCKINNEY, JOHN
Dean, Graduate School
Duke University
Durham, North Carolina 27706
(919) 684-8111

MACALUSO, ANN
Assistant Director for Process
Studies
Commission on Federal Paperwork
1111 Twentieth Street, NW
Washington, D.C. 20582
(202) 254-9666

MANEY, ANN
Senior Research Sociologist
Mental Health Study Center
2340 University Blvd. East
Adelphi, Maryland 20852
(301) 436-6278

MICHAEL, DONALD
Center for Research on the
Utilization of Scientific Knowledge
Institute for Social Research
The University of Michigan
Ann Arbor, Michigan 48106
(313) 764-2554

MITROFF, IAN
Prof. of Information Science
University of Pittsburgh
Room 738 - Info. Sci. Bldg.
Pittsburgh, Penn. 15260
(412) 624-5204

NEWELL, WILLIAM
Professor of Management
University of Washington
Seattle, Washington 98195
(206) 543-4898

POZIOMEK, EDWARD
Mid-Atlantic Regional Director
Sigma Xi, The Scientific Research
Society of North America
1411 Valley Stream Road
Bel Air, Maryland 21014

QUEEN, WILLIAM
Dept. of Botany
H. J. Patterson Hall
University of Maryland
College Park, Maryland 20742
(301) 454-3823

RETTIG, RICHARD A.
The RAND Corporation
2100 M Street, NW
Washington, D.C. 20037
(202) 296-5000

RICH, ROBERT
Center for Research on the
Utilization of Scientific Knowledge
The Institute for Social Research
The University of Michigan
Ann Arbor, Michigan 48106
(313) 764-2554

ROOT, VERNON
Applied Physics Laboratory
Johns Hopkins Road
Laurel, Maryland 20810
(301) 953-7100, ext. 2111

ROSE, DAVID J.
Dept. of Nuclear Engineering
MIT 24-210
Cambridge, Mass. 02139
(617) 253-3807

ROSENSTEIN, ALLEN
School of Engineering and
Applied Science
UCLA
Los Angeles, Calif. 90024
(213) 825-2537

ROSSINI, FREDERICK J.
Asst. Prof. of Social Sciences
Georgia Institute of Technology
Atlanta, Georgia 30332
(404) 894-3195

RUGG, LESLIE
9832 Kincardine Avenue
Los Angeles, Calif. 90034
(213) 836-0238

SALASIN, SUSAN
Chief, Research Diffusion and
Utilization
National Institute of Mental Health
5600 Fishers Lane, Room 11C09
Rockville, Maryland 20852
(301) 443-3767

SCHUELKE, DAVID
Center for Research in
Scientific Communication
Dept. of Rhetoric, 202 Haecker Hall
University of Minnesota
St. Paul, Minnesota 55108
(612) 373-0817

SHAH, SALEEM
Chief, Center for Studies of Crime
and Delinquency
National Institute of Mental Health
5600 Fishers Lane
Rockville, Maryland 20852
(301) 443-3728

SNOW, JOEL
National Science Foundation
Room P-705
1900 Pennsylvania Avenue, NW
Washington, D.C. 20550
(202) 634-4017

SPARROW, F. TOMLINSON
Prof. of Economics and Industrial
Engineering
3801 Cullen Blvd.
University of Houston
Houston, Texas 77004
(719) 749-3272

TAYLOR, JAMES B.
School of Social Welfare
The University of Kansas
Lawrence, Kansas 66045
(913) 864-4720

TEICH, ALBERT
Associate Professor
Graduate Program on Science,
Technology and Public Policy
George Washington University
Washington, D.C. 20052
(202) 676-7292

WARING, JOHN A.
Research Writer and Consultant
8502 Flower Avenue
Takoma Park, Maryland 20012
(202) 695-2205

WEINER, ARTHUR
Dept. of Human Relations and
Counseling Studies
University of Waterloo
Waterloo, Ontario
N2L3G1 CANADA
(519) 885-3913

WEISS, CAROL
Bureau of Applied Social Research
605 W. 115th Street
New York, New York 10025
(212) 280-4051

WOLF, CHARLES P.
Office of Technology Assessment
119 D Street, NE
Washington, D.C. 20510
(202) 224-7044

WOODROW, RAYMOND J.
Assistant to the President
Princeton University
P. O. Box 36
Princeton, New Jersey 08540
(609) 452-3096

WRIGHT, CHRISTOPHER
Professional Staff
R & D Planning and Priorities
Program
Office of Technology Assessment
U.S. Congress
Washington, D.C. 20510
(202) 224-1801

Observers:

PICKETT, BETTY
Acting Director, Division of
Extramural Research
National Institute of Mental Health
5600 Fishers Lane Room 10105
Rockville, Maryland 20851
(301) 443-3563

CUTLER, ROBERT
Research Management Improvement
Program
National Science Foundation
1800 G Street, NW
Washington, D.C. 20550
(202) 632-5826

POWERS, ERNEST
Division of Policy Research and
Analysis
AD/STIA
National Science Foundation
1800 G Street, NW
Washington, D.C. 20550
(202) 632-4144

AAAS Staff:

SCRIBNER, RICHARD A.
Manager, Special Programs
AAAS
1776 Massachusetts Avenue, NW
Washington, D.C. 20036
(202) 467-4475

CHALK, ROSEMARY A.
Staff Officer
AAAS
1776 Massachusetts Ave., NW
Washington, D.C. 20036
(202) 467-5436

Professional Society Representatives:

Institute of Electrical and Electronic Engineers (IEEE)
Harold Chestnut, General Electric

The Institute of Management Sciences (TIMS)
Arie Y. Lewin, Duke University

Operations Research Society of America (ORSA)
Burton V. Dean, Case Western Reserve University

Sigma Xi, The Scientific Research Society of North America
Edward Poziomek, Aberdeen Laboratory

Society for General Systems Research (SGSR)
Clair Blong , SGSR

Society for Technical Communication (STC)
Vernon M. Root, Applied Physics Laboratory, Johns Hopkins

APPENDIX B

ADAPTING SCIENCE TO SOCIAL NEEDS: KNOWLEDGE, INSTITUTIONS, PEOPLE INTO ACTION

A AAAS Workshop/Conference; May 5-8, 1976; Institute of Man and Science

AGENDA

Wednesday Evening, May 5

5:00 PM Register and Cocktails

7:30 PM Dinner

8:30 PM KEYNOTE ADDRESS

C. West Churchman, Schools of Business Administration,
University of California at Berkeley

Towards A Holistic Approach

Thursday Morning, May 6

8:30 AM Opening Remarks and Plan of the Conference: R. Scribner

CONFERENCE PAPERS SET I: WORKING ON THE PROBLEMS

1. David J. Rose, Prof. of Nucl. Eng., MIT

   The Energy Problem: Fragmented, Resource-Specific Approaches Don't Work

   Commentator: Joseph Leary, ERDA

2. Ann C. Maney, Senior Research Sociologist, Mental Health Study Center, NIMH

   The NIMH Experience in Social Problems Research: Theoretical Issues and Organizational Structures

   Commentator: Clark C. Abt, President, Abt Associates

3. Kenneth W. Heathington, Director, Transportation Center University of Tennessee

   Urban Transportation: The Real Issues Need to Be Addressed

4. Christopher Wright, private consultant (former Dir. of the Inst. for the Study of Science in Human Affairs, Columbia Univ.)

   Applying Science to Public Problems: The Emerging Structure of Interdisciplinary Efforts

AGENDA

Thursday Afternoon, May 6

12:30 PM Lunch

Free Time

3:00 PM WORKSHOPS SET I: ASSESSING THE PROBLEMS (WORKSHOPS A-F)

6:00 PM Workshops close

6:30 PM Dinner

7:30 PM PLENARY SESSION: REPORTS FROM WORKSHOPS AND DISCUSSION

9:30 PM Close of Session

Friday Morning, May 7

8:30 AM CONFERENCE PAPERS SET II: ORGANIZATION AND PROCESS

5. Don E. Kash, Director, Science and Public Policy Program,
The University of Oklahoma

Observations on Interdisciplinary Studies and Government Roles

Commentator: Joel Snow, National Science Foundation

6. Henry Lambright, Associate Professor of Political Science
Syracuse University, and
Vaughn Blankenship, Head, Office of Planning and Policy Analysis
National Science Foundation

University Research Centers: A Comparison of the NASA and RANN Experiences

Commentator: Al Teich, State University of New York, Albany

7. Nathan Caplan, Program Director, Center for Research on the
Utilization of Scientific Knowledge, University of Michigan

Utilization of Problem-Oriented Research: By Whom? For What?

Commentator: Saleem Shah, Chief, Center for Studies in Crime and Delinquency, NIMH

AGENDA

Friday Afternoon, May 7

12:30 PM Lunch

Free Time

3:30 PM WORKSHOPS SET II: RECOMMENDING ACTIONS (WORKSHOPS G-K, SEE PAGE 4)

6:00 PM Workshops close

6:30 PM Dinner

7:30 PM PLENARY SESSION: REPORTS FROM WORKSHOPS AND DISCUSSION

9:30 PM Close of Session

Saturday Morning, May 8

9:00 AM PLENARY SESSION: OBSERVATIONS ON CONFERENCE RESULTS

1. Donald Michael, Program Head, Center for Research on the Utilization of Scientific Knowledge

   Loosening the System for Risk Taking: Creating a 'Charitable' Environment for Responsible and Imaginative Approaches to Problem-Oriented Collaboration

2. Walter A. Hahn, Senior Specialist in Science and Technology, Congressional Research Service

   Observations on Interdisciplinarity: Its Need, Management and Utilization

3. Daniel Alpert, Director, Center for Advanced Study, University of Illinois

   The Dilemmas Facing Us

4. C. West Churchman, Center for Research in Management Science, University of California at Berkeley

   Action Alternatives for AAAS

AGENDA

WORKSHOPS

Thursday (Assessing)

A. "Conceptual Difficulties in Problem-Oriented Research: How to Formulate the 'Holistic' Question"

Chairperson: Charles Wolf
Reporter: Fred Rossini

B. "Motivation and Reward Structures: What Are the Incentives and Risks in Doing Problem-Oriented Research?"

Chairperson: Ronald Corwin
Reporter: Sherry Arnstein

C. "Problem-Oriented Research Projects: Leadership, Management, Communication Factors"

Chairperson: Leslie Rugg
Reporter: Raymond Woodrow

D. "Alternative Organizational Designs to Meet Social Needs: The Generation and Use of Science in Solving Public Problems"

Chairperson: Arie Lewin
Reporter: Ian Mitroff

E. "Roles for and Linkages among Science Institutions in More Effectively Helping to Solve Social Problems"

Chairperson: Joel Snow
Reporter: Daniel Alpert

F. "What Is the Demand for Problem-Oriented Work? Who (Individuals, Institutions) Perceives that They Need Interprofessional Collaboration?"

Chairperson: Richard Bolt
Reporter: Clark Abt

Friday (Recommending)

G. "Unresolved Conceptual Questions About Science and Social Problems: Is More Research Needed?"

Chairperson: Leonard Goodwin
Reporter: Robert Knapp

H. "Recommendations for Improving Motivation and Reward Structures: Changing the System"

Chairpersom: Don Michael
Reporter: James Taylor

I. "Recommendations for Creating Effective Management Styles for Interdisciplinary Research: What Are the Unique Factors?"

Chairperson: Daniel Horvitz
Reporter: Norman Evans

J. "Recommendations for New Organizational Designs: Adapting Old Institutions to New Functions"

Chairperson: Tom Glennan
Reporter: Gerald Gordon

K. "Affecting the Environment for Problem-Oriented Research: Government Funding, Agency Attitudes, Public Markets"

Chairperson: Tom Sparrow
Reporter: Edward Poziomek

APPENDIX C

CONFEREES EVALUATE THE CONFERENCE

At the end of the Workshop/Conference two of the participants, Leonard Goodwin and Suzanne Brainard, formulated questions to obtain some information about people's feelings on the conference. The questionnaire is shown below, the analysis follows on subsequent pages.

COMMENTS: FEELINGS ABOUT THE CONFERENCE

During yesterday's dinner discussion with Dick Scribner, it seemed to us useful to obtain some information about people's feelings on the conference experience. Listed below are four questions. Your answers could help evaluate the kind and quality of the personal interactions, and how a conference can contribute along these lines.

Responses are of course anonymous (if you wish) but the overall analysis, which we will carry out, will be sent to each conference participant.

L. Goodwin, S. Brainard

1. What events at the conference made you feel good?

2. What events made you feel uncomfortable?

3. What were the most important things (if any) that happened to you at the conference?

4. What would you have done differently, if you were to redesign the conference?

5. Further comments:

WORCESTER
POLYTECHNIC
INSTITUTE

Worcester
Massachusetts 01609
(617) 753-1411

20 May 1976

Dr. Richard A. Scribner, Director
Office of Special Programs
American Association for the
Advancement of Science
1776 Massachusetts Avenue, N.W.
Washington, D.C. 20036

Dear Dick and Rosemary:

Enclosed is the content analysis of the Comments on the conference, and the responses themselves. I presume you will send copies of the analysis to participants.

The comments indicate a very good feeling about the conference, but I do not detect a consensus on next steps. Concern about involving a more diverse group in the discussions I think is valid. However, this would intensify the problem already apparent, namely, there are marked differences of viewpoint among persons in the governmental, social scientific, natural scientific, etc. worlds.

In order to create a common basis for communication among relevant persons concerned with a given social need or problem, it may be necessary for them to share some common experiences in examining the nature of social problems and the role of scientific effort in elucidating them.

More specifically, a relevant group of people concerned about a social issue (whether energy, health, welfare/employment) might begin by exploring the differing ways in which important organizations perceive that issue and the reasons why. They might also consider the scope and limits of scientific efforts to analyze the issue and the role of other forms of understanding such as artistic creation. Then members of the group might be able to interrelate their expertise in a fashion that leads to a new understanding of and a new approach to the issue.

I look forward to any further materials that might emerge from the conference, and if I can be of further help please let me know.

Sincerely,

Leonard

Leonard Goodwin, Head
Dept. of Social Science
and Policy Studies

LG/jl

enclosures

ANALYSIS OF "COMMENTS" MADE BY PARTICIPANTS

AAAS CONFERENCE ON ADAPTING SCIENCE TO SOCIAL NEEDS

MAY 5-8, 1976; INSTITUTE ON MAN AND SCIENCE

L. Goodwin
S. Brainard

The following analysis is based upon responses of 26 participants in the conference. While there were about 70 participants altogether, only about 40 were present at the end of the conference when the questionnaire was distributed. Whether the results are representative of the entire group cannot be determined, but at least there is information from a substantial number of participants. The content analysis itself appears on the next pages.

HIGHLIGHTS

Responses indicate a strong positive feeling about the conference as the result of getting new ideas, communicating with persons across disciplinary boundaries, etc. (see Items 1, 3 and 5). At the same time, there is some concern that interdisciplinary communication is inadequate to the job to be done in meeting social problems (see Item 2).

There is a strong concern (Item 5) for going beyond the initial conference and doing something more in this interdisciplinary social need area. Responses to Items 4 and 5 indicate a desire to broaden the spectrum of people involved, to synthesize ideas and to make recommendations for future steps. How this desire is to be translated into action is not altogether clear, although responses to Item 4 give a number of suggestions about conference organization.

* * *

Content analysis for Item 1: What events at the conference made you feel good?

Topics	Responses* Number	Responses* Percent
1. General good feeling about the conference--exchanged ideas, met interesting people.	13	28
2. The final (Saturday) session--excellent presentations	7	15
3. The prepared papers, or one of the plenary speeches	7	15
4. The workshops	6	13
5. Discussion of interdisciplinary issues	5	11
6. Informal discussion, free time to meet people	5	11
7. Discussion of moral and value issues	3	6
Total	47	99%

Content analysis for Item 2: What events made you feel uncomfortable?

1. Intellectual arrogance of different groups (e.g. government, academic) and barriers to new thinking	8	25
2. General feeling of mismatch between what scientific community can do and what needs to be done; lack of understanding of scientific and interdisciplinary issues	8	25
3. The plenary sessions--too long, too many	4	13
4. Presentation of workshops' conclusions	4	13
5. Lack of constructive closure at workshops	3	9
6. Not enough time for problem-solving	2	6
7. Summary session in evening	1	3
8. Inadequate representation at conference from different groups	1	3
9. Did not learn purpose of the conference	1	3
Total	32	100%

* Multiple answers to an item by an individual are coded as separate responses.

Content analysis for Item 3: What were the most important things that happened to you at the conference?

Topics	Responses*	
	Number	Percent
1. Opportunity to meet interesting people	16	43
2. Learned something--new insights	11	30
3. Realization that there is a community sharing my concerns	5	14
4. Beginning interdisciplinary network of communication	2	5
5. Don't know yet	1	3
6. Opportunity to relax	1	3
7. Saturday summary	1	3
Total	37	101%

Content analysis for Item 5: Further Comments

1. Should broaden participation base in continuing interdisciplinary efforts of this kind	11	42
2. Appreciated participating in this venture--learned a great deal, excellent staff work, will develop interdisciplinary network	10	40
3. AAAS should produce an edited work of the conference, form a standing committee on topic, sponsor visiting lecturers at institutions in this area	4	15
4. A danger in trying to institutionalize charity	1	4
Total	26	101%

---

* Multiple answers to an item by an individual are coded as separate responses.

Content analysis for Item 4: What would you have done differently, if you were to redesign the conference?

	Topics	Responses*	
		Number	Percent
1.	Invite broader spectrum of people	7	15
2.	Devote more time on synthesis of ideas and recommendations of future steps	7	15
3.	Reduce number, length and reading of formal talks	6	13
4.	More time for informal discussion	4	9
5.	Increase workshop time	3	7
6.	Opportunity to follow-up activities of conference	3	7
7.	Permit self-organizing workshops	2	4
8.	Narrow scope of issues to be discussed	2	4
9.	Clearly specify problem and scope of workshop activities	2	4
10.	Have a smaller number of participants	2	4
11.	Nothing	2	4
12.	Clearly define goal of conference	2	4
13.	Eliminate summary sessions	1	2
14.	Begin with small group discussions	1	2
15.	Locate in metropolitan area	1	2
16.	Devote evening sessions to small groups	1	2
	Total	46	98%

*Multiple answers to an item by an individual are coded as separate responses.

APPENDIX D

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